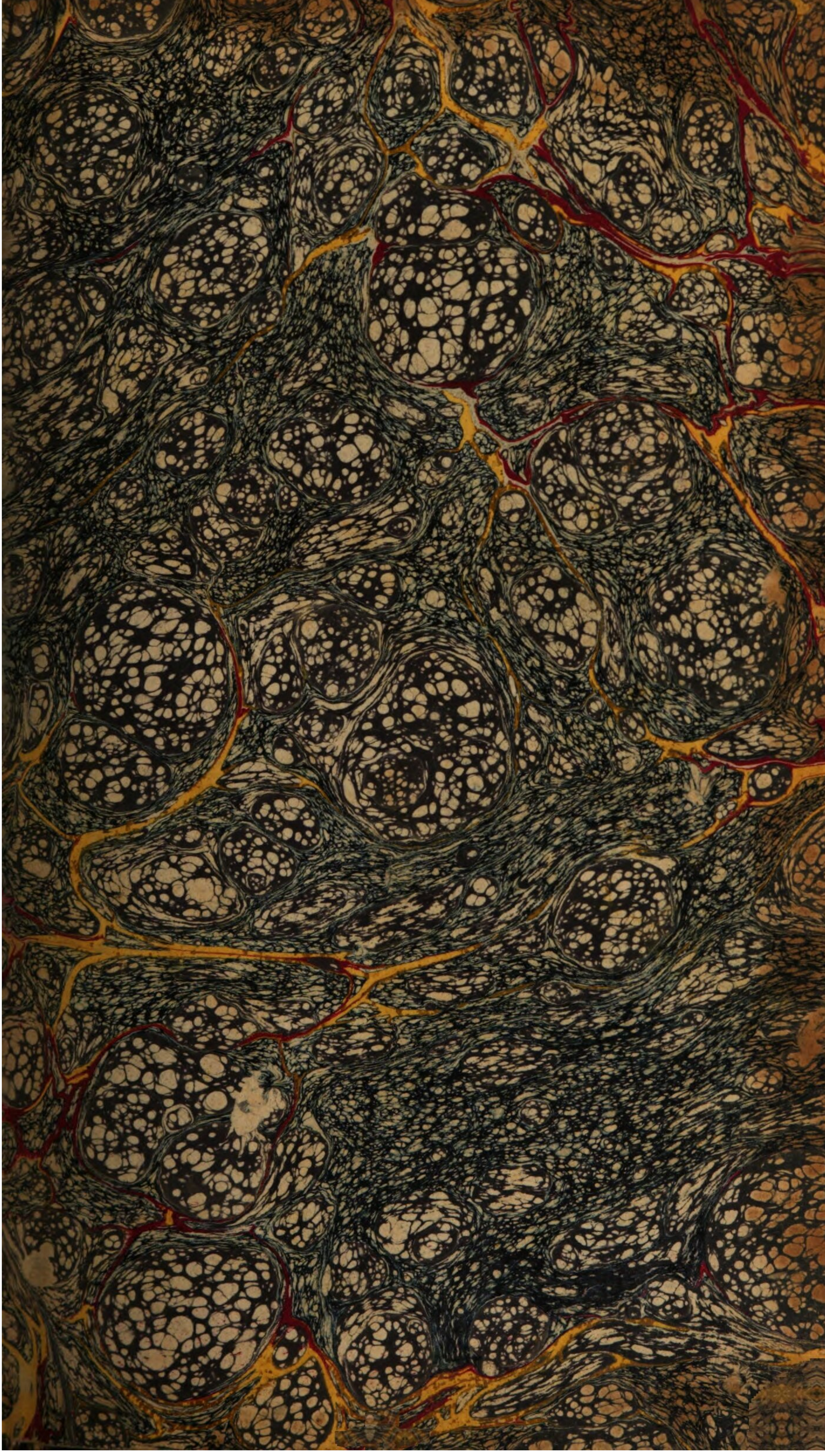

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Gemahlt von Kellerhofen in München 1792 gestochen v. Rauschmayr.

COUNT RUMFORD

PHILOSOPHICAL PAPERS:

BEING A COLLECTION OF

MEMOIRS, DISSERTATIONS,

AND

EXPERIMENTAL INVESTIGATIONS,

RELATING TO VARIOUS BRANCHES OF

NATURAL PHILOSOPHY AND MECHANICS.

TOGETHER WITH

LETTERS TO SEVERAL PERSONS ON SUBJECTS CONNECTED WITH SCIENCE AND USEFUL IMPROVEMENT.

BY

BENJAMIN COUNT OF RUMFORD,
LL.D. F.R.S. &c. &c.

VOL. I.

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DEDICATION.

TO HIS MOST SERENE HIGHNESS

MAXIMILIAN JOSEPH,

ELECTOR PALATINE OF BAVARIA.

SIR,

AS it is natural for an Author to be anxious that his Works ſhould come before the Public under the moſt reſpectable auſpices, I may conſider myſelf as being moſt ſingularly fortunate in having obtained permiſſion to dedicate this Book to YOUR MOST SERENE ELECTORAL HIGHNESS.

I am ſtill more fortunate in having found a generous Benefactor and a Friend in the Succeſſor of the Munificent Prince in

whoſe ſervice many years of the moſt active period of my life have been ſpent.

I cannot, indeed, forget that it was to Your Moſt Serene Electoral Highneſs I was originally indebted for the diſtinguiſhed reception I met with at the Court of Munich, on my firſt arrival there, in the year 1783; and conſequently for all the kindneſs I afterwards received from your ILLUSTRIOUS PREDECESSOR.

If, during the fourteen years I had the honor to ſerve HIS MOST SERENE HIGHNESS, Your late UNCLE, I endeavoured, by a faithful diſcharge of my duty, to deſerve the confidence with which he never ceaſed to honor me; and to merit ſome of thoſe diſtinguiſhed marks of his favor which he was pleaſed to confer on me, yet ſtill I muſt ever feel myſelf greatly indebted to YOUR MOST SERENE ELECTORAL HIGHNESS, not only for your kindneſs to me when I firſt had the honor of being preſented to you—for the flattering recommendation you gave me when I firſt

went to Munich;—and for the kind aſſurances you gave me of your eſteem, protection, and friendſhip, on your ſucceeding to your preſent Bavarian Dominions, on the death of your late UNCLE, my kind friend and benefactor; but I am bound to you ſtill more, if it be poſſible, by the flattering invitation you have lately given me to come to you, and reſide at your Court, and aſſiſt in the great work of carrying into execution the vaſt plans you have formed for promoting the proſperity of your ſubjects.

The public proofs which your ELECTORAL HIGHNESS has already given of your earneſt deſire to render your ſubjects enlightened, induſtrious, rich, and happy, afford the moſt favourable preſages of the future proſperity of Bavaria, and of ſuch other Countries, as have, and as may have the good fortune to make parts of your Dominions.

That you may long reign over a flouriſhing, and a grateful People, is the ardent

wiſh of him who has the diſtinguiſhed honor to be, with the moſt profound reſpect, and ſincereſt gratitude and attachment,

Your Moſt Serene Electoral Highneſs's

Devoted Servant,

RUMFORD.

London,
MAY 1, 1802.

PREFACE.

MOST of the Papers contained in this Volume have already appeared in the Tranſactions of the Royal Society of London; and ſome of them have been tranſlated into foreign languages; yet, as in this Publication I have carefully reviſed and corrected each of thoſe Papers; and as I have added Notes and Supplements to ſeveral of them, I flatter myſelf that the Volume will not be altogether unintereſting, or unworthy a place in the Libraries of thoſe who collect books of this kind.

The ſecond Volume, which will conſiſt chiefly of original Letters, written on various ſcientific ſubjects, and on uſeful inventions and improvements, will, no doubt, be generally thought more intereſting.

I have it in contemplation to publiſh, at ſome future period, a work, (printed uniformly with this Volume, and with my Eſſays,) on Original Mechanical Inventions; which work, together with the continuation of my Eſſays, and the Memoirs of my Life, (which laſt, it is probable, will not be publiſhed till after my death,) will be all the Publications I have at preſent any intention of giving to the world.

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IX. CONJECTURES reſpecting the PRINCIPLES of the HARMONY of COLOURS. (From page 333 to page 340.)

It appears to be highly probable that the two colours which are exhibited by two ſhadows, in experiments with coloured ſhadows, are in all caſes ſuch, that if they could be intimately mixed together, the reſult of that mixture would be *perfect whiteneſs*; and as theſe two colours are always found to harmonize, or to afford to the eye the moſt ſatisfactory and pleaſing contraſt, we may perhaps venture to conclude that two colours are then, and only then in *perfect harmony*, when they, together, contain all the colours, in their due proportions, to form whiteneſs; in which caſe one of the colours may, with propriety, be ſaid to be the *complement* of the other.—If this conjecture be well founded, it will be eaſy, in all caſes, to find out what colour harmonizes

harmonizes with any given colour; and alſo to aſcertain the preciſe degree of ſtrength, or brilliancy, which this harmonizing colour ought to have, 333—335.—Painters might derive great advantages from a knowledge of the *principles* of the harmony of colours, 336.—An account is given of an experiment in which *imaginary colours* were called up, or exhibited, without the aſſiſtance of ſhadows—precautions that are neceſſary in making experiments of this kind, 337—339.—Advantages which painters may derive from theſe diſcoveries.—Great importance of the *ſcience* of painting—until it is more cultivated the *art of colouring* muſt continue to be very obſcure and uncertain, 340.

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XI. SUPPLEMENT to the foregoing Paper. (From page 363 to page 365.)

An account is given in this ſupplement of an experiment in which the oxide of gold was reduced without heat by means of an aqueous ſolution of gum arabic, expoſed ſeveral weeks in the moderate light of a cloſet fronting the north, where the direct rays of the ſun never entered.—The reſult of this experiment was peculiarly intereſting on account of the manner in which the glaſs phial which contained this mixture was tinged with colours, and beautifully gilded, 363.—An account is given of another experiment in which a ſlip of ivory which had been ſoaked in an aqueous ſolution of nitro-muriate of ſilver was firſt turned black, and then ſilvered, on being expoſed in a tumbler of cold water to the action of the ſun's direct rays, 364.—Probability that this new method of ſilvering ivory may turn out to be a valuable acquiſition to the arts, 365.

XII. AN ENQUIRY concerning the WEIGHT or PONDERABILITY that has been ASCRIBED TO HEAT. (From page 366 to page 384.)

The experiments which have hitherto been made with a view to determining whether bodies do in fact become either heavier or lighter on being heated, are of a nature ſo delicate, and are liable to ſo many errors, that no reliance can be placed on their reſults, eſpecially as thoſe reſults have been extremely various, and often contradictory.—The author, happening to have in his poſſeſſion

poſſeſſion an excellent balance, and every thing elſe at hand that was neceſſary to the proſecution of this curious and intereſting inveſtigation, undertakes (in the winter of the year 1787), a ſet of experiments of which an account is given in this paper.—The apparatus made uſe of in theſe experiments is deſcribed in the moſt particular manner; and all the precautions are pointed out which were found to be neceſſary in order to prevent errors from adventitious circumſtances.—From the nature of the ſubject it is evident that an abſtract, or ſhort analyſis of this paper could convey no ſatisfactory information to the reader.—The reſult of theſe enquiries may, however, be given in a few words:—The experiments ſeem to ſhew that *all attempts to diſcover any effects of heat on the apparent weights of bodies, will be fruitleſs.*

XIII. SUPPLEMENT to the foregoing Paper. (From page 384 to page 390.)

In this ſupplement an account is given of ſeveral experiments which were made with iron bullets, and with a ball of pure gold; which were weighed, ſucceſſively, at different temperatures, in a very accurate balance.—They were ſometimes weighed when they were at the temperature of freezing water, or below that point, and were afterwards weighed when they were perfectly red-hot, and ſometimes when they were near the temperature at which they would have been diſpoſed to melt.—The iron bullet was weighed ſixteen times ſucceſſively at different intervals of time, while it was paſſing from a ſtate of the moſt intenſe heat, and gradually cooling down to the moderate temperature of the air of the atmoſphere.—Some remarkable changes were obſerved in the apparent weight of the iron bullet while it was cooling, but theſe appearances were found to depend on a particular circumſtance, (the gradual oxidation of the metal)—and conſequently they cannot be adduced as a proof that the apparent weights of bodies are either increaſed or diminiſhed by heat.—A compariſon of the reſult of the experiments made with the iron bullet, with the reſult of thoſe made with the ball of pure gold will put that queſtion beyond all doubt; and will alſo tend much to confirm the general concluſion drawn from the reſults of all the experiments of which an account is given in the foregoing paper; namely, that HEAT IS IMPONDERABLE.—Whether it be in fact IMPONDEROUS, or void of all weight, is left to others to determine.

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ACCOUNT

OF

SOME EXPERIMENTS UPON GUN-POWDER,

With occaſional and practical Inferences; to which are added, an Account of a new Method of determining the Velocities of all kinds of Military Projectiles; and the deſcription of a very accurate Eprouvette for Gun-powder.

[Read before the Royal Society, March 29, 1781.]

THESE experiments were undertaken principally with a view to determine the moſt advantageous ſituation for the vent in fire-arms, and to meaſure the velocities of bullets, and the recoil under various circumſtances. I had hopes alſo of being able to find out the velocity of the inflammation of gun-powder; and to meaſure its force more accurately than had hitherto been done. They were begun in the month of July in the year 1778, at Stoneland Lodge, a country-ſeat belonging to Lord GEORGE GERMAIN; and I was aſſiſted by the reverend Mr. BALE, rector of Withyham, who lives in the neighbourhood.

The weather proved remarkably favourable for our experiments, being ſettled and ſerene, ſo that the courſe of them was never interrupted for a whole day by rain or by any accident. The mercury in the barometer ſtood in general pretty high, and the temperature of the atmoſphere was very equal and moderately warm for the ſeaſon. In order that each experiment might, as nearly as poſſible, be made under ſimilar circumſtances, they were all made between the hours of ten in the morning and five in the afternoon: and after each experiment the piece was wiped out with tow till the inſide of its bore was perfectly clean, and as bright as if it had juſt come out of the hands of the maker; and great care was taken to allow as much time to elapſe between the firings, as was neceſſary to render the heat of the barrel nearly the ſame in every experiment.

A deſcription of the apparatus.

The barrel principally uſed in theſe experiments was made by WOGDON, one of the moſt famous gunſmiths in London; and nothing can exceed the accuracy with which it is bored, or the fineneſs of the poliſh on the inſide. It is made of the very beſt iron, and, agreeably to Mr. ROBINS's advice, I took care to have it well fortified in every part, that there might be no danger of its burſting. Its weight and dimenſions may be ſeen in the table of the weight and dimenſions of the apparatus, p. 16.

Fig. 1. Repreſents a longitudinal ſection of a part of the barrel, with the apparatus firſt made uſe of for ſhifting the vent from one part of the chamber to another, or rather for moving the bottom of the chamber further from, or bringing it nearer to, the vent, in order that the fire might be communicated to the powder in different parts of the charge.

a, *b*, repreſent the lower part of the barrel.

c, is the breech-pin, which is perforated with a hole four tenths of an inch in diameter, the axis of which perforation coincides with the axis of the bore.

Into this hole the ſcrew *h*, *n*, about four inches in length, is fitted; to the end of which, *n*, that paſſes up into the bore, is fixed a piſton *o*, *p*, which, by means of collars of oiled leather, is made to fit the bore of the piece very exactly. The end of the piſton *p*, neareſt the muzzle, is of braſs, and forms a moveable bottom to the bore, which by turning the ſcrew *h*, *n*, by means of the handle *m*, is brought nearer to, or removed further from, the fixed vent *v*, by which means the powder is lighted at any aſſignable diſtance from the bottom of the charge.

But the length of the bore being altered by moving the piſton, which occaſioned a ſmall inaccuracy, and ſome inconvenience attending the apparatus, it was laid aſide, and another repreſented by fig. 2. was ſubſtituted in the room of it.

a, *b*, is a ſection of part of the barrel as before, and *c* is the breech-pin, which being perforated with a

ſmall hole through its center receives the ſcrew *f*, *g*, which is about two-tenths of an inch in diameter, and four inches long. This ſcrew being perforated with a very ſmall hole, ſerves to convey the fire into the chamber of the piece, and by ſcrewing it further up into the bore, or drawing it backwards, the fire is communicated to different parts of the charge.

But this method being found to be not entirely free from inaccuracies and inconveniences, a third was ſubſtituted in the room of it, which was found to anſwer much better than either of the preceeding.

The end of the bore was now firmly cloſed by a ſolid breech-pin, *p*, fig. 3. and three vent holes *m*, *n*, and *o*, were made in the barrel; one of them, *m*, even with the bottom of the bore, and the other two at different diſtances from it. Any two of theſe vent holes, as *n* and *o* for inſtance, being cloſed up by ſolid ſcrews, a perforated ſcrew, or vent tube *v*, was ſcrewed into the third, which ſerved to contain the priming, and to convey the fire to the powder lodged in the bore of the piece.

Sometimes a longer vent-tube, repreſented by fig. 4. was made uſe of; which, paſſing through the powder in the chamber of the piece, communicated the fire immediately to that part of the charge that lay in the axis of the bore.

Another vent-tube alſo was uſed occaſionally, which differs in many reſpects from both thoſe that have been deſcribed. It is ſo conſtructed as to convey the fire to the charge; but, as ſoon as the powder in the chamber of the piece begins to kindle,

kindle, and the elaſtic fluid to be generated, the vent is firmly cloſed by a valve, and no part of the generated fluid is permitted to eſcape. This I ſhall call the *valve-vent*, and it is repreſented by fig. 5. upon an enlarged ſcale, that the parts of it may appear more diſtinct.

a, *b*, is a longitudinal ſection of a ſmall portion of the ſolid ſide of the barrel.

c, *d*, is the vent-tube, which is in all reſpects like the ſhort vent-tube commonly made uſe of, except only that in this the end of the vent-hole (*c*) which goes into the chamber is enlarged in the form of the wide end of a trumpet or funnel.

To this enlarged aperture the valve *v*, is accurately fitted, and by means of the ſmall ſtem or tail, *t*, which is fixed to the valve, and which paſſes up through the vent-hole, and is connected with the ſpring S, the valve is preſſed, or rather drawn into its place, and the vent is cloſed. The ſtem of the valve was at firſt made cylindrical; but, in order to make way for the priming to paſs down to the valve, one half of its ſubſtance was taken away, as is repreſented in the figure.

When this vent is primed, the ſpace between the vent-hole and the ſtem of the valve is filled with fine grained powder, and the valve is gently opened by preſſing upon the end of the ſtem till one or more grains of powder lodge themſelves between the valve and the aperture; which preventing the valve from cloſing again, a ſmall opening is left for the paſſage of the flame into the chamber of the piece; when the priming is lighted, the

fire paſſing down the vent, and entering the chamber inflames the charge, and the ſmall grains of powder that were lodged between the valve and the aperture being deſtroyed by the flame in its paſſage through the vent, the valve immediately cloſes, and prevents the eſcape of any part of the elaſtic fluid generated by the inflammation of the powder in the chamber of the piece. The preſſure of this fluid upon the valve aſſiſts the action of the ſpring, by which means the valve is more expeditiouſly and more effectually cloſed.

The valve was very accurately fitted to the aperture by grinding them together with powdered emery, and afterwards poliſhing them one upon the other. And it is very certain, that no part of the elaſtic fluid made its eſcape by this vent; for, upon firing the piece, there was only a ſimple flaſh from the exploſion of the priming, and no ſtream of fire was to be ſeen iſſuing from the vent, as is always to be obſerved when a common vent is made uſe of, and in all other caſes where this fluid finds a paſſage.

In order that every part of the apparatus employed in theſe experiments might be as perfect as poſſible, all the more delicate parts of it were executed by Mr. Frazer, mathematical inſtrument-maker in Duke's Court, St. Martin's Lane, and, among the reſt, all the contrivances juſt deſcribed relative to the vent.

The velocities of the bullets were determined by means of a pendulum, according to the method invented by Mr. Robins.

The

The pendulum I made uſe of (fig. 6.) is compoſed of a circular plate of hammered iron (*a*), 13 inches in diameter, and 0,65 of an inch thick, to which is firmly faſtened a bar of iron (*b*, *c*,) 56,5 inches in length, 2,6 inches broad, and half an inch in thickneſs, by which it is ſuſpended by means of two pivots (*d*, *e*,) at the end of the bar (*c*), and at right angles to its length. Theſe pivots being very accurately finiſhed, and moving on poliſhed grooves, which were kept conſtantly oiled to leſſen the friction, the vibration of the pendulum was very free, as appeared by the great length of time its vibrations continued after it had been put in motion, and was left to itſelf. To the circular plate of the pendulum, targets of circular pieces of wood of different thickneſſes were fixed, which in the courſe of the experiments were often ſpoiled and replaced; and, in order to mark the weight and dimenſions of the pendulum in each experiment, the pendulums are numbered according to the different targets that were made uſe of; and the weight and dimenſions of each pendulum are ſet down in a table at the end of the deſcription of the apparatus.

The target of the pendulum N° 1. was made of a circular piece of elm-plank $3\frac{1}{2}$ inches thick, and equal in diameter to the iron plate of the pendulum to which it was fixed; but this target being too thin was very ſoon ruined.

The pendulum N° 2. was furniſhed with two targets, which were circular pieces of very tough oak-plank, near five inches thick, placed on oppoſite

ſides of the plate of the pendulum, and firmly fixed to it by ſcrews, and to each other by iron ſtraps. When one of theſe targets was ruined, the pendulum was turned about, and the other was made uſe of. This pendulum laſted from experiment N° 9, to experiment N° 39. when it was ſo much ſhattered as to be rendered unfit for further ſervice.

The pendulum N° 3. was like N° 2.; only, inſtead of oak, elm-plank near ſeven inches in thickneſs was made uſe of for the targets. This pendulum ſerved from experiment N° 40. to experiment N° 101. incluſively.

But finding that targets made of planks of the tougheſt wood were very ſoon ſhattered to pieces by the bullets, I compoſed the pendulum N° 4. in a different manner. Inſtead of circular pieces of plank, ſolid cylinders of elm-timber were made uſe of for the targets, ſo that the bullets now entered the wood in the direction of its fibres. Theſe cylinders are 13 inches in diameter, and about $5\frac{1}{2}$ inches in length, hooped with iron at both their ends, to prevent their ſplitting, and they are firmly faſtened to the plate of the pendulum, and to each other by four iron ſtraps. This pendulum laſted till the experiments were finiſhed. It is ſtill in being, and appears to be very little the worſe for the ſervice it has undergone.

Fig. 7. ſhews the two ends of the pendulum upon a large ſcale, together with the hooks by which it was ſuſpended.

a, *b*, is the bar of the pendulum, which is ſeen broken off, as there is not room to ſhew the whole of its length.

c, *d*,

c, *d*, are the pivots by which it was ſuſpended.

e, is the circular plate of the pendulum, to which *f*, *g*, two circular targets, are faſtened by ſcrews, and by means of the iron ſtraps, 1, 2, 3, 4, which are nailed to the edges of the targets.

h, *k*, are the hooks which ſerved to receive the pivots, *c*, *d*, of the pendulum.

The hooks were firmly fixed to the horizontal beam R, S, which ſupported the whole apparatus by means of three ſcrews *m*, *n*, *o*, which paſſed through three holes in the plate that connects the two hooks. When the hooks are faſtened to the beam, the middle ſcrew, *n*, was firſt put into its place, and the pendulum was allowed to ſettle itſelf in a poſition truly vertical, after which the hooks were immoveably fixed by means of the ſcrews *m*, *o*.

The chord of the arc, through which the pendulum aſcended in each experiment, was meaſured by a ribbon, according to the method invented and deſcribed by Mr. ROBINS.

The recoil was meaſured in the following manner. The barrel was ſuſpended in an horizontal poſition (and nearly in a line with the center of the target) by two ſmall pendulous rods, 64 inches in length, and 25,6 inches aſunder; which being parallel to each other, and moving freely upon poliſhed pivots about the axes of their ſuſpenſion, and upon two pair of trunnions that were fixed to the barrel, formed, together with the barrel, a compound pendulum; and from the lengths of the vibrations of this pendulum, the velocity with which

the

the barrel began to recoil, or rather its greateſt velocity, was determined.

But in order that the velocity of the recoil might not be too great, ſo as to endanger the apparatus, when large charges were made uſe of, it was found neceſſary to load the barrel with an additional weight of more than 40 lbs. of iron.

This additional weight of iron, which I ſhall call the *gun-carriage*, as it was ſo conſtructed as to ſerve as a carriage to the barrel, is compoſed of a bar of hammered iron 28 inches in length, 2,6 inches broad, and half an inch in thickneſs, which is bent in the middle of its length in ſuch a manner, that its two flat ſides or ends are parallel to each other, and diſtant aſunder two inches. In the middle of this bar where it is bent is a hole in the form of an oblong ſquare, which, receiving the end of the breech-pin, ſupports the lower end or breech of the barrel. The other end of the barrel is ſupported and confined in the following manner. A ring or hoop of iron, near half an inch thick, and two inches in diameter, is placed in a vertical poſition between the parallel ſides of the bar, and near its two ends, and firmly fixed to them by ſcrews. The barrel paſſing through the middle of this ring is ſupported upon the ends of three ſcrews, which paſſing through the ring in different parts of its circumference all point towards its centre.

The carriage, together with the barrel, was ſuſpended by the pendulous rods by means of two pair of poliſhed trunnions that are fixed to the out-

ſide

ſide of the carriage. They are placed in an horizontal line perpendicular to, and paſſing through, the axis of the bore.

Fig. 8. repreſents the barrel fixed to the carriage.

a, *b*, *c*, is the bar of iron ſeen edge-ways, which forms the carriage.

2, 2, 4, 4, are the trunnions by which it was ſuſpended.

d, *e*, is the barrel in its proper place.

p, is the breech-pin, which paſſing through a hole in the middle of the bar, *a*, *b*, *c*, ſupports the end, *e*, of the barrel; and

n, is the ring that ſupports the end, *d*, of the barrel.

Fig. 9. repreſents a perpendicular ſection through the line 2, 2, fig. 8. and in a line perpendicular to the length of the barrel.

This figure is deſigned to ſhew the manner in which the muzzle of the piece was ſupported and confined in the ring *n*, fig. 8.

a, *c*, are the two ends of the bar that are ſeen cut off.

n, is the ring, and

o, *p*, are the ſcrews by which it is faſtened to the two parallel ſides of the bar, the ends of which ſcrews form the trunnions 2, 2, fig. 8.

d, is a tranſverſe ſection of the barrel, and

r, *s*, *t*, are the three ſcrews by which the barrel is ſupported and confined in the center of the ring.

Fig. 10. is the ſame as fig. 9. but upon a larger ſcale, and without the letters of reference.

Fig. 11. repreſents the two ends of one of the pendulous

pendulous rods by which the barrel was ſuſpended; and fig. 13. ſhews the ſame ſeen ſide-ways.

a, *b*, is the rod which is ſeen broken off.

c, *d*, are the pivots by which it was ſuſpended by a pair of hooks that were faſtened to an horizontal beam, in the ſame manner as the pendulum for meaſuring the velocities of the bullets was ſuſpended.

e, *f*, are the hooks which receive the trunnions that are fixed to the carriage.

The dimenſions of every part of this apparatus may be ſeen in the table, page 16.

The chord of the arc through which the barrel aſcended in its recoil was meaſured by a ribbon, and the lengths of thoſe chords, expreſſed in inches and decimal parts of an inch, are ſet down in the tables. The method of computing the velocity of the recoil from the chord of the arc through which the barrel aſcended, is too well known to require an explanation: and it is alſo well known, that the velocities are to each other as the chords of thoſe arcs. The lengths of thoſe chords, therefore, as they are ſet down in the tables, are, in all caſes, as the volocities of the recoil.

The powder made uſe of in theſe experiments was of the beſt kind, ſuch as is uſed in proving great guns at Woolwich. A cartridge, containing 12 lbs. of this powder, was given to me by the late General DESAGULIERS of the Royal Artillery, and Inſpector of Braſs and Iron Ordnance; who alſo, in the politeſt manner, offered me every other aſſiſtance in his power towards completing the experiments

ments I had projected, or in making any others I ſhould propoſe that might be uſeful in the proſecution of my inquiries.

This powder was immediately taken out of the cartridge, and put into glaſs bottles, which were previouſly made very clean and dry; and in theſe it was kept carefully ſealed up till it was opened for uſe. When it was wanted for the experiments, it was weighed out in a very exact balance, with ſo much attention, that there could not poſſibly be an error in any inſtance greater than one quarter part of a grain. The bottles were never opened but in fine weather, and in a room that was free from damp, and no more charges of powder than were neceſſary for the experiments of the day were weighed out at a time. Each charge was carefully put up in a cartridge of very fine paper, and theſe filled cartridges were kept in a turned wooden box, that was varniſhed on the inſide as well as the outſide, to prevent its imbibing moiſture from the air.

The paper of which theſe cartridges were made was ſo fine and thin, that 1280 ſheets of it made no more than an inch in thickneſs, and a cartridge capable of containing half an ounce of powder weighed but three quarters of a grain.

The cartridges were formed upon a wooden cylinder, and accurately fitted to the bore of the piece, and the edges of the paper were faſtened together with paſte made of flour and water.

When a cartridge was filled, the powder was gently ſhaken together, and its mouth was tied up and ſecured with a piece of fine thread; and when

it

it was made uſe of it was put intire into the piece, and gently puſhed down into its place with the ram-rod, and afterwards it was pricked with a priming-wire thruſt through the vent, and the piece was primed; ſo that no part of the powder of the charge was loſt in the act of loading, as is always the caſe when the powder is put looſe into the barrel; nor was any part of it expended in priming; but the whole quantity was ſafely lodged in the bottom of the bore or chamber of the piece, and the bullet was put down immediately upon it, without any wadding either between the cartridge and the bullet, or over the bullet.

The bullets were all caſt in the ſame mould, and conſequently could not vary in their weights above two or three grains at moſt, eſpecially as I took care to bring the mould to a proper temperature as to heat before I began caſting; and when leather was put about them, or other bullets than thoſe of lead were made uſe of, the weight was determined very exactly before they were put into the piece.

The diameter of the bullet was determined by meaſurement, and alſo by computation, from its weight, and the ſpecific gravity of the metal of which it was formed; and both theſe methods gave the ſame dimenſions very nearly,

The apparatus was put up for making the experiments in a coach-houſe, which was found very convenient for the purpoſe, as the joiſts upon which the floor over head was laid afforded a firm and commodious ſupport for ſuſpending the pendulum

and

and the barrel, and the walls and roofs of the building ſerved to ſcreen the apparatus, which otherwiſe might have been diſturbed by the wind, and injured by the rain and dews. A pair of very large doors, which formed the whole of one end of the room, were kept conſtantly open during the time the experiments were making, in order to preſerve the purity of the air within the houſe, which otherwiſe would have been much injured by the ſmoke of the gun-powder; which might poſſibly have had ſome effect in leſſening the force of the powder, and vitiating the experiments. In order ſtill further to guard againſt this evil, the barrel was placed as near as poſſible to the door, and the pendulum was hung up at the bottom of the room.

Fig. 12. repreſents the apparatus as it was put up for making the experiments.

a, *b*, is the barrel with its carriage, ſuſpended by the pendulous rods *c*, *d*, and

R, is the ribbon which ſerved to meaſure the aſcending arc of its recoil.

P, is the pendulum, and

r, the ribbon that meaſured the arc of its vibration.

The diſtance from the mouth of the piece to the pendulum was juſt 12 feet.

A Table ſhewing the weights and dimenſions of all the principal parts of the apparatus.

Of the barrel.

	Inches.
Length - - - - -	44,7
Length of the bore from the muzzle to the breech-pin - - -	43,45
Diameter of the bore - - -	0,78
Thickneſs of metal at the lower vent	0,36
Thickneſs of metal at the muzzle	0,1

Weight of the barrel, together with the ſolid breech-pin, and the vent-ſcrews and vent-tube, 6 lbs. 6 oz.

Of the gun-carriage.

Length - - - - -	28,4
Diſtance between the two pair of trunnions	25,6
Diameter of each trunnion - -	0,25

Weight 40 lbs. 14 oz.

Of the rods by which the carriage was ſuſpended.

Length from the axis of ſuſpenſion, or center of the pivots, to the center of the trunnions of the gun-carriage, 64 inches.

Weight of each rod, 1 lb. 4 oz.

Total weight of the barrel and its carriage, together with the allowance that was made for the weight of the rods by which it was ſuſpended, 48 lbs.

N. B. This was its weight from experiment N° 3. to experiment N° 123. incluſive.

Of

Of the bullet.

Diameter 0,75 of an inch.
Weight in lead 580 grains.

Of the pendulum.

	Inches.
Total length of the pendulum from the axis of ſuſpenſion to the bottom of the circular plate - - -	69,5
Diameter of the circular plate to which the targets were faſtened - -	13,
Diſtance between the ſhoulders of the pivots - - - - -	3,8
Diameter of the pivots - - -	,27

Weight of the iron part of the pendulum 47 lb. 4 oz.

Of the pendulum with the targets fixed to it, as it was prepared, for making the experiments.

	Total length to the ribbon.	Diſtance from the axis of ſuſpenſion.		Total weight of iron and wood.
		To the center of gravity.	To the center of oſcillation.	
	Inches.	Inches.	Inches.	lbs. oz.
Pendulum N° 1	69,25	50,25	58,45	57 0
——— N° 2	69,5	54,4	59,15	82 4
——— N° 3	———	55,62	60,23	100 12
——— N° 4	———	54,6	59,18	88 4

N. B. The meaſure is Engliſh feet and inches; and the weight is avoirdupois.

Having now gone through with the deſcription of all the principal parts of the apparatus, I ſhall proceed to give an account of the experiments. And as it may be ſatisfactory to ſee the method of conducting theſe enquiries, as well as the reſult of them, I ſhall firſt give a Table of the experiments in the exact order in which they were made, together with my original remarks; I ſhall then make ſuch general obſervations as may occur; and afterwards I ſhall ſelect, combine, and compare them, in the manner which may beſt anſwer the different purpoſes to which I ſhall apply them.

General

General Table of the Experiments.

In the two firſt experiments the barrel was fixed to a carriage (that has not been deſcribed) which, together with the barrel and rods by which it was ſuſpended, weighed only 23½ lbs.

Length of the bore of the piece, 43,5 inches.
Weight of the bullet, 580 grains.

The pendulum N° 1.

Order of experiments. N°	The charge of powder. Weight. Grs.	The charge of powder. Height. In.	Vent from the bottom of the charge. In.	Chord of the aſcending arc of the pendulum. Inches.	The bullet ſtruck the target below the axis of the pendulum. Inches.	Chord of the arc of the recoil. In.	Velocity of the bullet. Ft. in ſec.	Remarks.
1	208	1,8	0,	13,2	64,5	33,5	1267	*Firſt day.*
2	. .	. .	,5	14,5	. . .	36,5	1399	

This gun-carriage being found to be too light, the other, deſcribed above, and repreſented fig. 8, was ſubſtituted in the room of it.

Order of experiments. N°	The charge of powder. Weight. Grs.	Height. In.	Vent from the bottom of the charge. In.	Chord of afcending arc of the pendulum. In.	The bullet ſtruck the target below the axis of the pendulum. Inches.	Chord of the arc of the recoil. In.	Velocity of the bullet. Ft.in ſec.	Remarks.
3	208	1,8	0	12,6	65,	17,8	1213	*Second day.*
4	. .	. .	,5	——	——	18,5	—	The pendulum gave way.
5	. .	. .	0	——	——	38,68	—	4 bullets were fired at once.
6	. .	. .	,5	——	——	38,48	—	Ditto.
7	. .	. .	0	——	——	6,1	—	Without any bullet.
8	416	3,6	. .	——	——	16,5	—	Ditto.
9	208	1,8	0	8,5	65,	17,69	1281	Pen. N° 2; very fair 3*d. day.*
10	104	,9	. .	5,2	65,25	10,18	782	
11	310	2,7	0	9,6	64,6	24,69	1459	The powder was lighted by the long vent-tube (fig. 4.)
12	. .	. .	1,22	10,1	65,	24,95	1527	
13	. .	. .	2,65	11,85	64,75	24,9	1801	
14	. .	. .	. .	10,9	65,25	. .	1646	
15	330	2,9	2,65	10,9	61,5	26,2	1748	
16	. .	. .	. .	13,25	63,5	. .	2060	
17	330	2,7	2,65	——	——	12,7	——	The barrel very much heated.
18	. .	2,9	0	10,4	63,5	26,3	1619	
19	. .	. .	. .	. .	63,	26,4	1633	
20	165	1,45	0	6,8	62,2	14,73	1084	
21	. .	. .	. .	6,85	. . .	14,2	1093	
22	. .	. .	1,32	6,7	. . .	14,8	1071	
23	. .	. .	. .	6,3	60,6	14,58	1035	The ſhort vent-tube (*v*, fig. 3.) was made uſe of.
24	. .	. .	. .	7,5	61,5	14,68	1142	

In order to determine how much of the force of the powder was loſt by *windage*, and by the *vent*, oiled leather was faſtened round the bullet, ſo that it now accurately fitted the bore of the piece; and in the five experiments, from N° 35. to N° 39. inclusive, the valve-vent was made uſe of.

Weight of the bullet, together with the leather in which it was enveloped, 603 grains.

Order of experiments.	Weight of the charge of powder.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
25	165	1,45	0	6,8	65,	14,95	1004	
26	. .	. .	. .	7,8	. . .	15,6	1153	
27	. .	. .	. .	8,05	. . .	16,15	1192	*Fourth day.*
28	330	2,9	. .	10,2	63,	26,	1559	
29	. .	. .	2,6	. . .	64,	28,1	1536	
30	165	3,2	. .	5,9	62,4	13,2	914	
31	. .	1,45	1,3	6,65	62,6	15,15	1027	

Finding that the blaſt of the powder always reached as far as the pendulum, when large charges were uſed, and ſuſpecting that this circumſtance, together with the impulſe of the unfired grains, might in a great meaſure occaſion the apparent irregularity in the velocities of the bullets; to remedy theſe inconveniences, a large ſheet of paper of a moderate thickneſs was ſtretched upon a ſquare frame of wood, and interpoſed as a ſcreen before the pendulum, at the diſtance of two feet from the ſurface of the target.

Two reaſons conſpired to induce me to prefer this method of preventing the impulſe of the flame upon the pendulum to the obvious one of removing the pendulum further from the mouth of the piece; the firſt was, that I was unwilling to increaſe the diſtance between the barrel and the pendulum, leſt

the reſiſtance of the air might affect the velocities of the bullets; and the ſecond, which I confeſs did not operate leſs ſtrongly than the firſt, was, that the length of the houſe did not admit of a greater diſtance, and I was unwilling to expoſe any part of the apparatus in the open air.

But the ſcreen was found to anſwer perfectly well the purpoſe for which it was deſigned, and it was continued during the remainder of the experiments; the paper being replaced every third or fourth experiment.

The Experiments continued.

N°	Order of experiments.	The charge of powder. Weight.	The charge of powder. Height.	Vent from the bottom of the charge.	Chord of aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
		Grs.	In.	In.	In.	Inches.	Inch	Ft.in ſec.	
32		165	1,45	0	5,45	63,	15,45	839	Not leathered; weight of the bullet and wad 603 grs. In exp. No 32. no leſs than 40 large of unfired powder were driven through the ſcreen.
33		. .	. .	. .	. . .	. . .	12,65	839	
34		. .	. .	. .	7,9	. . .	15,45	1217	In theſe 6 experiments the bullets were leathered, and the powder was lighted by the valve-vent. The pend. N°2. ruined.
35		. .	. .	. .	7,	60,25	15,25	1129	
36		. .	. .	. .	7,4	62,	16,3	1161	
37		. .	. .	1,3	8,	61,	17,9	1277	
38		290	2,6	2,6	9,	58,6	23,5	1497	
39		. .	. .	. .	. . .	. . .	24,8	. .	

The bullets were now put naked into the piece, and the powder was lighted by the ſhort vent-tube (*v.* fig. 3.) and ſome little improvement was made in the ſteel edges between which the ribbons paſſed that ſerved to meaſure the aſcending arcs of the pendulum and of the recoil, by which means the friction was leſſened, and the ribbon was prevented from twiſting or entangling itſelf as it was drawn out.

Apparatus.

The barrel with its carriage as before.
The pendulum, N° 3. and
Leaden bullets, weighing 580 grains each.

Order of experiments. N°	The charge of powder. Weight.	The charge of powder. Height.	Vent from the bottom of the charge.	Chord of afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Grs.	In.	In.	Inch	Inches.	Inch.	Ft. in ſec.	
40	218	1,9	0	6,45	64,6	18,	1236	*5th day*; mean velocity in theſe exper. and N° 47. 1225.
41	. .	. .	. .	6,31	65,3	17,71	1197	
42	. .	. .	. .	6,45	65,	17,91	1230	
43	. .	. .	1,3	6,5	64,6	18,3	1248	
44	. .	. .	. .	6,75	64,5	18,35	1299	Mean velocity 1276
45	. .	. .	. .	6,6	64,9	. . .	1265	
46	. .	. .	. .	6,4	61,6	. . .	1293	
47	. .	. .	0	6,3	62,	18,1	1266	
48	290	2,6	0	7,2	63,5	22,58	1414	Mean velocity 1427
49	. .	. .	. .	7,4	. . .	22,92	1455	
50	. .	. .	. .	7,3	64,6	22,38	1412	
51	290	2,6	1,3	7,4	63,	23,21	1476	Mean velocity 1493
52	. .	. .	. .	7,6	64,	23,76	1520	
53	. .	. .	. .	7,25	61,	23,6	1483	
54	. .	. .	2,6	7,5	62,3	. . .	1502	Mean velocity 1460
55	.	. .	. .	7,4	64,	23,26	1450	
56	. .	. .	. .	7,1	62,2	. . .	1433	
57	. .	. .	. .	7,4	64,	23,56	1454	
58	. .	. .	. .	1,31		11,12	—	In theſe 4 experiments the piece was fired with powder alone, and the ſcreen was taken away from before the pendulum.
59	. .	. .	. .	1,2		11,62	—	
60	. .	. .	0	1,16		9,62	—	
61	. .	. .	1,3	0,6		11,33	—	

Order

Order of experiments.	The charge of powder.							
	Weight.	Height.	Vent from the bottom of the charge.	Chord of afcending arc of the pendulum.	Bullet ſtruck the tar- get below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
N°	Grs.	In.	In.	In.	Inches.	Inch.	Ft.in ſec.	
62	330	2,9	1,3	8,	63,	26,4	1599	6*th day*; mean velo-
63	. .	. .	. .	8,5	65,	. . .	1652	city 1625.
64	. .	. .	2,6	7,2	59,5	25,3	1562	Mean velocity 1528.
65	. .	. .	. .	7,7	65,	. . .	1495	
66	. .	. .	0	8,4	. . .	26,35	1633	Mean velocity 1594.
67	. .	. .	. .	8,	. . .	25,8	1556	
68	218	1,9	0	6,82	64,	19,56	1349	powder was rammed very hard.
69	. .	. .	. .	6,6	64,6	18,2	1294	Ditto much harder.
70	. .	. .	. .	6,85	. . .	19,12	1345	Ditto as hard as in N° 68.
71	. .	. .	1,3	5,5	. . .	16,33	1080	Ditto, ditto.
72	. .	. .	0	——	——	8,72	—	Government powder, no bullet.
73	. .	. .	. .	——	——	8,44	—	Beſt double battle powder.
74	. .	. .	1,3	——	——	8,47	—	Government powder.
75	. .	. .	. .	——	——	9,3	—	Double battle powder.

The following experiments, N°s 78, 79, 80, and 81, were made in hopes of being able to diſcover a method of adding to the force of gunpowder. *Twenty grains* of the ſubſtances mentioned in the remarks upon each experiment were intimately mixed with the powder of the charge. In the experiment N° 82. a large wad of tow, well ſoaked in etherial ſpirit of turpentine, was put into the piece immediately upon the bullet: and in the experiment N° 83. a wad, ſoaked in alkohol, was put into the piece in like manner.

Order

Order of experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	In.	Inches.	Inch	Ft.in ſec.	
76	145	1,3	0	5,3	65,	13,25	1037	7th day; medium velocity 1040.
77	..	..	..	...	64,6	13,25	1044	
78	..	..	..	3,2	...	8,92		20 grs. beſt alkaline ſalt of tartar
79	..	..	..	4,35	...	11,68		20 grs. æthiops mineral.
80	..	..	..	3,3	63,6	9,83		20 grs. ſal ammon.
81	..	..	..	4,2	63,4	11,45		20 grs. fine braſs duſt.
82	..	..	..	—	——	15,25	—	The ſcrews which held the hooks by which the pendulum was ſuſpended gave way, and the pendulum came down.
83	..	..	..	—	——	14,35	—	

In the nine following experiments, *viz.* from N° 84. to N° 92. incluſive, the valve-vent was made uſe of, and the bullets were made to fit the bore of the piece very exactly by means of oiled leather, which was ſo firmly faſtened about them that in each experiment it entered the target with the bullet.

The bullet made uſe of in experiment N° 85. was of wood.

Thoſe uſed in the experiments N° 86. and N° 87. were formed in the following manner; a ſmall bullet was caſt of plaiſter of Paris, which being thoroughly dried, and well heated at the fire, was

fixed

fixed in the center of the mould that ſerved for caſting all the leaden bullets uſed in theſe experiments; and melted lead being poured into this mould, the cavity that ſurrounded the ſmall plaiſter bullet was intirely filled up, and a bullet was produced, which to the eye had every appearance of ſolidity, but was as much lighter than a ſolid leaden bullet of the ſame diameter as the ſmall plaiſter bullet was lighter than a leaden bullet of the ſame ſize.

In the experiments N° 88. and N° 89. ſolid leaden bullets were made uſe of. In the experiment N° 90. *two* bullets were diſcharged at once; in the experiment N° 91. *three;* and in the experiment N° 92. *four* were uſed.

In each of theſe experiments a freſh ſheet of paper was made uſe of as a ſcreen to the pendulum, in order that the velocities of the bullets might be meaſured more accurately; and alſo, that the quantity of unfired powder might be eſtimated with greater preciſion.

Order

Order of experiments.	The charge of powder. Weight.	The charge of powder. Height.	Vent from the bottom of the charge.	Weight of the bullet.	Chord of afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of recoil.	Velocity of the bullet.	Remarks.
N°	Grs.	In.	In.	Grs.	In.	Inches.	In.	Ft.in ſec.	
84	145	1,3	0	—	—	—	4,5	—	8*th day*; in each of theſe 4 experiments from 50 to 70 granulæ or particles of unfired powder were driven through the ſcreen.
85	. .	. .	. .	90	1,33	62,2	7,16	1763	
86	. .	. .	. .	251	2,82	63,2	9,62	1317	
87	. .	. .	. .	354	3,32	61,2	11,3	1136	
88	. .	. .	. .	600	6,5	65,4	15,22	1229	Very few unfired grains of powder ſtruck the ſcreen.
89	. .	. .	. .	603	6,3	64,6	15,13	1229	
90	. .	. .	. .	1184	10,12	65,	21,92	978	There were no marks of any unfired powder having reached the ſcreen.
91	. .	. .	. .	1754	13,65	63,4	27,18	916	
92	. .	. .	. .	2352	16,55	63,3	32,25	833	

In the ſeven following experiments the piece was fired with powder only.

Order

Order of experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Chord of the arc of the recoil.	Remarks.
N°	Grs.	In.	In.	Inches.	Inches.	
93	145	1,3	0	——	4,3	
94	165	1,45	. .	——	5,5	
95	. .	. .	. .	——	5,6	
96	290	2,6	. .	——	11,70	
97	437½	3,9	. .	1,68	17,5	The screen was taken away.
98	. -	. .	. .	6,7	15,88	The whole surface of the target was bespattered with unfired grains of powder.
99	. -	. .	. .	——	17,9	The pendulum was not observed.

In the following experiments, N° 100. and N° 101. the bullets were not put down into the bore, but were supported by three wires, which being fastened to the end of the barrel projected beyond it, and confined the bullet in such a situation that its center was in a line with the axis of the bore, and its hinder part was one-twentieth of an inch without or beyond the mouth of the piece.

In experiment N° 102. the bullet was just stuck into the barrel in such a manner that near one-half of it was without the bore.

Order

Order of experiments.	The charge of powder.	Weight.	Height.	Vent from the bottom of the charge.	Chord of afcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
N°		Grs.	In.	In.	In.	Inches.	In.	Ft.in ſec.	
100		165	1,45	0	,65	60,5	4,9	138	In each of theſe experiments near one-tenth part of the ſubſtance of the bullet was melted and blown away by the impulſe of the flame.
101		. .	. .	. .	,43	uncertain	4,8	92	
102		. .	. .	. .	,86	63,	5,6	180	

All that part of the bullet which lay towards the bore of the piece appeared to be quite flat from the loſs of ſubſtance it had ſuſtained; and its ſurface was full of ſmall indents, which probably were occaſioned by the unfired grains of powder that impinged againſt it.

The

The following experiments were made with the pendulum N° 4. The reſt of the apparatus as before.

Order of experiments. N°	The charge of powder. Weight. Grs.	The charge of powder. Height. In.	Vent from the bottom of the charge. In.	Chord of aſcending arc of the pendulum. In.	Bullet ſtruck the target below the axis of the pendulum. Inches.	Chord of the arc of the recoil. Inches.	Velocity of the bullet. Ft.in ſec.	Remarks.
								9th. day.
103	104	,9	0	4'51	65,	10,6	732	About 40 grains of powder were driven through the ſcreen.
104	145	1,3	. .	5,4	. . .	12,92	877	About 40 unfired grs. of powder.
105	. .	. .	. .	5,6	. . .	13,28	910	Mean velocity 894. 40 unfired grains.
106	. .	1,14	. .	6,18	65,8	14,3	990	Double proof battle powder; on unfired grains.
107	218	1,8	. .	8,48	65,	19,68	1380	Ditto, ditto
108	290	2,6	. .	9,45	65,6	23,9	1526	Government powder; bullet leathered; weight 602 grains.
109	. .	. .	. .	873	65,2	22,8	1419	Bullet naked; very few unfired grains.
110	. .	. .	. .	9,3	65,6	23,4	1460	Mean velocity 1444
111	. .	. .	. .	. .	. . .	. . .	1462	
112	. .	. .	. .	8,85	65,5	22,94	1436	
113	. .	. .	2,6	8,65	64,	23,7	1438	Medium velocity 1413.
114	. .	. .	. .	8,5	63,6	24,1	1423	
115	. .	. .	. .	8,4	65,	23,8	1378	
116	. .	2,28	. .	9,15	64,	24,6	1525	Double proof battle powder.
117	437½	3,9	. .	10,56	64,9	33,	1738	Gov. pow. No unfired grains.
118	. .	. .	. .	11,	64,5	33,3	1824	thro' the ſcreen.
119	. .	. .	. .	10,5	65,	33,6	1729	Mean velocity, 1764.
120	. .	. .	2,6	10,35	. . .	32,5	1706	
121	. .	. .	. .	10,65	. . .	33,2	1757	Mean velocity 1751.
122	. .	. .	. .	10,6	63,6	32,9	1789	
123	. .	. .	0	——	——	17,9	—	Without any bullet.

Of the method made use of for computing the velocities of the bullets.

As the method of computing the velocity of a bullet, from the arc of the vibration of a pendulum into which it is fired, is so well known, I shall not enlarge upon it in this place, but shall just give the theorems that have been proposed by different authors, and shall refer those who wish to see more on the subject to Mr. ROBINS's New Principles of Gunnery; to Professor EULER's Observations upon Mr. ROBINS's Book; and, lastly, to Dr. HUTTON's Paper on the initial Velocities of Cannon Balls, published in the Transactions of the Royal Society for the year 1778.

If a, denote the length from the axis of the pendulum to the ribbon which measures the chord of the arc of its vibration;

g, the distance of the center of gravity below the axis of the pendulum;

f, the distance of the center of oscillation; and

b, the distance of the point struck by the bullet from that axis;

c, the chord of the ascending arc of the pendulum;

P, the weight of the pendulum;

b, the weight of the bullet, and

v, the original velocity of the bullet;

$v = \frac{c}{a} \times \frac{\overline{Pg}}{\overline{bb}} + \frac{\overline{b}}{\overline{f}} \times \frac{f}{\sqrt{2b}}$, is a theorem for finding the velocity upon Mr. ROBINS's principles.

*$v = \frac{c}{a} \times \overline{\frac{Pg}{bh}} + \overline{\frac{f+h}{2f}} \times \sqrt{\frac{f}{2}}$, is the theorem proposed by Professor EULER, who has corrected a small error in Mr. ROBINS's method; and

$v = 5{,}672\,cg \sqrt{f} \times \frac{P+b}{bha}$ is Dr. HUTTON's theorem, which is sufficiently accurate, and far more simple and expeditious than either of the preceding. It is to be remembered, that g, h, and c, may be expressed in any measure; but f must be English feet, and v will be the velocity of the bullet in English feet in a second.

The velocities of the bullets in most of the foregoing experiments were first computed by EULER's method, as I had not then seen Dr. HUTTON's paper; but in going over the calculations a second time, I made use of Dr. HUTTON's theorem. Both these methods gave the same velocity very nearly, but the Doctor's method is by much the easiest in practice.

In these computations care was taken to make a proper allowance for the bullets that were lodged in the pendulum, and also for the velocity lost by the bullet in passing through the screen.

The corrections necessary on account of the bul-

* Put the rational part $\frac{c}{a} \times \overline{\frac{Pg}{bh}} + \overline{\frac{f+h}{2f}} = n$, and express f in thousandth parts of a Rhynland foot; then the velocity with which the ball strikes the pendulum will be $= \frac{n}{4} \sqrt{\frac{f}{2}}$ Rhynland feet in a second.

lets lodged in the pendulum were made in the following manner.

b was continually added to the value of P,

$\frac{b-g}{P} \times b$ . . to the value of g, and

$\frac{f-b}{P} \times b$ . . to the value of f.

Of the ſpaces occupied by the different charges of powder.

The heights of the charges of powder, or the lengths of the ſpaces which they occupied in the bore, were determined by meaſurement; and in order that this might be done with greater accuracy, inches and tenths of inches were marked upon the ram-rod, and the charge was gently forced down till it occupied the ſame ſpace in each experiment.

The following table ſhews the heights of the charges as they were determined by meaſurement, and alſo their heights computed from the diameter of the bore of the piece, and the ſpecific gravity of the powder that was uſed.

N. B. By an experiment of which I ſhall give an account hereafter, I found the ſpecific gravity of this powder, ſhaken well together, to be to that of rain water, as 0,937 is to 1,000.

Weight of the powder.	Height of the charge	
	Measured.	Computed
Grs.	Inches.	Inches.
104	,9	0,8957
145	1,3	1,2490
165	1,45	1,4211
208	1,8	1,7914
218	1,9	1,8775
290	2,6	2,4980
310	2,7	2,6700
330	2,9	2,8422
416	3,6	3,5828
437½	3,9	3,7680

In the experiment N°30. the powder was put into a cartridge so much smaller than the bore of the piece, that the charge, instead of occupying 1,45 inches, extended 3,2 inches. By this disposition of the powder, its action upon the bullet appears to have been very much diminished.

Of the effect that the HEAT *which pieces acquire in being fired, produces upon the force of powder.*

It is very probable, that the excess of the velocity of the bullet in the second experiment, over that of the first, was occasioned more by the heat which the barrel had acquired in the first experiment, than by the position of the vent, or any other circumstance; for I have since found, upon repeated trials, that the force of any given charge of powder is considerably greater when it is fired in a piece that has been previously heated by firing, or by any other means, than when the piece has not been heated. Every body that is acquainted with artillery knows, that the recoil of great guns is much more violent after the second or third discharge, than it is at first; and on ship-board, where it is necessary to attend to the recoil of the guns, in order to prevent very dangerous accidents that might be occasioned by it, the constant practice has been in our navy, and, I believe, on board the ships of all

other nations, to leſſen the quantity of powder after the firſt four or five rounds: our 32 pounders, for inſtance, are commonly fired with 14 lbs. of powder at the beginning of an action, but the charge is very ſoon reduced to 11 lbs. and afterwards to 9 lbs. and the filled cartridges are prepared accordingly.

By the recoil, it ſhould ſeem, that the powder exerted a greater force alſo in the fourth experiment, being the ſecond upon the ſecond day, than it did upon the third, or the firſt upon that day; but the pendulum giving way, it was not poſſible to compare the velocities of the bullets in the manner we did in the two experiments mentioned above.

This augmentation of the force of powder, when it is fired in a piece that is warm, may be accounted for in the following manner. There is no ſubſtance we are acquainted with that does not require to be heated before it will burn; even gunpowder is not inflammable when it is cold. Great numbers of ſparks, or red-hot particles from the flint and ſteel are frequently ſeen to light upon the priming of a muſket, without ſetting fire to the powder, and grains of powder may be made to paſs through the flame of a candle without taking fire; and what is ſtill more extraordinary, if large grains of powder are let fall from the height of two or three feet upon a red-hot plate of iron, laid at an angle of about 45° with the plane of the horizon, they will rebound intire, without being burnt, or in the leaſt altered, by the experiment. In all theſe caſes the fire is too feeble, or the duration of its

action

action is not sufficiently long to heat the powder to that degree which is necessary in order to its being rendered inflammable.

Now as gun-powder, as well as all other bodies, acquires heat by degrees, and as some space of time is taken up in this, as well as in all other operations, it follows, that powder, which has been warmed by being put into a piece made hot by repeated firing, is much nearer that state in which it will burn, or, I may say, is more inflammable, than powder which is cold; consequently, more of it will take fire in a given short space of time, and its action upon the bullet and upon the gun will of course be greater.

The heat of the piece will also serve to dry the air in the bore, and to clear the inside of the gun of the moisture that collects there when it has not been fired for some time, and these circumstances doubtless contribute something to the quickness of the inflammation of the powder, and consequently to its force.

As it takes a longer time to heat a large body than a small one, it follows, that meal-powder is more inflammable than that which is grained; and the smaller the particles are, the quicker they will take fire. Sailors bruise the priming after they have put it to their guns, as they find it very difficult, without this precaution, to fire them off with a match: and if those who are fond of sporting would make use of a similiar artifice, and prime their pieces with meal-powder, they would miss fire less often,—the springs of the lock might be made

more tender, and its ſize conſiderably reduced without any riſque, and the violence of the blow of the flint and ſteel in ſtriking fire being leſſened, the piece might be fired with greater preciſion.

Concluding from the reſults of the four experiments mentioned above, as well as from the reaſons juſt cited, that the temperature of the piece has a conſiderable effect upon the force of the powder, I afterwards took care to bring the barrel to a proper degree of heat, by firing it once, or oftener with powder, each time I recommenced the experiments after the piece had been left to cool.

Of the manner in which pieces acquire heat in firing.

I was much ſurpriſed upon taking hold of the barrel immediately after the experiment N° 17. when it was fired with 330 grains of powder without any bullet, to find it ſo very hot that I could ſcarcely bear it in my hand, evidently much hotter than I had ever obſerved it before, notwithſtanding the ſame charge of powder had been made uſe of in the two preceding experiments, and in both theſe experiments the piece was loaded with a bullet, which, one would naturally imagine, by confining the flame, and prolonging the time of its action, would heat the barrel much more than when it was fired with powder alone.

I was convinced that I could not be miſtaken in the fact, for it had been my conſtant practice to take hold of the piece to wipe it out as ſoon as an experiment was finiſhed, and I never before had found

found any inconvenience from the heat in holding it. But in order to put the matter beyond all doubt, after letting the barrel cool down to the proper temperature, I repeated the experiment twice, with the ſame charge of powder and a bullet; and in both theſe trials (experiments N° 18. and N° 19.) the heat of the piece was evidently much leſs than what it was in the experiment above mentioned (N° 17.).

I now regretted exceedingly the loſs of a ſmall pocket thermometer, which I had provided on purpoſe to meaſure the heat of the barrel, but it was accidentally broken by a fall the day before I began my experiments; and being ſo far from London, I had it not in my power to procure another: I was therefore obliged to content myſelf with determining the heat of the barrel as well as I could by the touch.

Being much ſtruck with this accidental diſcovery of the great degree of heat that pieces acquire when they are fired with powder without any bullet, and being deſirous of finding out whether it is a circumſtance that obtains univerſally, I was very attentive to the heat of the barrel after each of the ſucceeding experiments; and I conſtantly found the heat ſenſibly greater when the piece was fired with powder only, than when the ſame charge was made to impel one or more bullets.

Though the reſult of theſe experiments was totally unexpected, and even contrary to what I ſhould have foretold, if I had been aſked an opinion upon the ſubject previous to making them; yet, after mature conſideration, I am now convinced, that it

is what ought to happen, and that it may be accounted for very well, upon principles that are clearly admiſſible.

It is certain, that a very ſmall part only of the heat that a piece of ordnance acquires in being fired is communicated to it by the flame of the powder; for the time of its action is ſo ſhort (not being, perhaps, in general longer than about $\frac{1}{200}$th or $\frac{1}{150}$th part of a ſecond) that if its heat, inſtead of being 4 times, as Mr. ROBINS ſuppoſes, was 400 times hotter than red-hot iron, it could not ſenſibly warm ſo great a maſs of metal as goes to form one of our large pieces of cannon. And beſides, if the heat of the flame were ſufficiently intenſe to produce ſo great an effect in ſo ſhort a time, it would certainly be ſufficient, not only to burn up all inflammable bodies that it came near, but alſo to melt the ſhot that it ſurrounded and impelled, eſpecially when they were ſmall, and compoſed of lead, or any other fuſible metal; but, ſo far from this being the caſe, we frequently ſee the fineſt paper come out of the mouth of a piece uninflamed, after it has ſuſtained the action of the fire through the whole of the bore, and the ſmalleſt lead ſhot is diſcharged without being melted.

But it may be objected here, that the bullets are always found to be very hot if they are taken up immediately after they come out of a gun; and, that this circumſtance is a proof of the intenſity of the heat of the flame of powder, and of its great power of communicating heat to the denſeſt bodies. But to this I anſwer, I have always obſerved the

ſame

ſame thing of bullets diſcharged from wind-guns and croſs-bows, eſpecially when they have impinged againſt any hard body, and are much flattened; and bullets from muſkets are always found to be hotter in proportion to the hardneſs of the body againſt which they are fired. If a muſket ball be fired into a very ſoft body, as (for inſtance) into water, it will not be found to be ſenſibly warmed; but if it is fired againſt a thick plate of iron, or any other body that it cannot penetrate, the bullet will be demoliſhed by the blow, and the pieces of it that are diſperſed about will be found to be in a ſtate very little ſhort of fuſion, as I have often found by experience. It is not by *the flame* therefore that bullets are heated, but by precuſſion. They may, indeed, receive ſome ſmall degree of warmth from the flame, and ſtill more perhaps by friction againſt the ſides of the bore, but it is in ſtriking againſt hard bodies, and from the reſiſtance they meet with in penetrating thoſe that are ſofter, that they acquire by far the greater part of the heat we find in them as ſoon as they come to be at reſt, after having been diſcharged from a gun.

There is another circumſtance that may poſſibly be brought as an objection to this opinion, and that is the running of the metal in braſs guns upon repeatedly firing them, by which means the vent is often ſo far enlarged as to render the piece intirely uſeleſs. But this, I think, proves nothing but that braſs is very eaſily corroded, and deſtroyed by the flame of the gun-powder; for it cannot be ſuppoſed, that in theſe caſes the metal is ever

fairly

fairly melted. The vent of a muſket is very ſoon enlarged by firing, and after a long courſe of ſervice it is found neceſſary to ſtop it up with a ſolid ſcrew, through the centre of which a new vent is made, of the proper dimenſions. This operation is called buſhing, or rather bouching the piece; but in all the better kind of fowling-pieces the vent is lined, or bouched, with gold, and they are found to ſtand fire for any length of time without receiving the leaſt injury. But every body knows that gold will run with a leſs heat than is required to melt iron; but gold is not liable to be corroded by any thing that can be produced in the combuſtion of gun-powder;—but this is not the caſe with iron; and that I take to be the only reaſon why a vent that is lined with gold is ſo much more durable than one that is made in iron. But it ſeems, that iron is more durable than braſs; and perhaps ſteel, or ſome other cheap metal, may be found that will ſupply the place of gold, and by that means the great expence that attends bouching pieces with that precious metal may be ſpared, and this improvement may be introduced into common uſe.

This leads us to a very eaſy and effectual remedy for that defect ſo long complained of in all kinds of braſs ordnance, *the running of the vent*; for if theſe pieces were bouched with iron, there is no doubt but they would ſtand fire as well as iron guns; and if ſteel, or any other metal, either ſimple or compounded, ſhould upon trial be found to anſwer for that purpoſe better than iron, it might

might be uſed inſtead of it; and even if gold were made uſe of for lining the vent, I imagine it might be done in ſuch a manner as that the expence would not be very conſiderable, at the ſame time that the thickneſs of the gold ſhould be ſufficient to withſtand the force of the flame for a very great length of time.

But to return to the heat acquired by guns in firing. It being pretty evident that it is not *all* communicated by the flame, there is but one other cauſe to which it can be attributed, and that is the *motion* and *friction* of the internal parts of the metal among themſelves, occaſioned by the ſudden and violent effort of the powder, upon the inſide of the bore, and to this cauſe I imagine the heat is principally if not almoſt entirely owing. It is well known, that a very great degree of heat may be generated in any hard and denſe body in a ſhort time by friction, and in a ſtill ſhorter time by colliſion. "For if two denſe hard elaſtic bodies be "ſtruck againſt each other with great force and "velocity, all the parts of ſuch bodies will every "moment be cloſely compreſſed, and being rigid "will re-act with equal force. Hence a quick and "powerful contraction and expanſion will ariſe in "every part, reſembling that ſwift kind of vibra-"tion obſerved in ſtretched ſtrings; how great "theſe vibrations are may be learnt from the in-"ſtance of a bell, when ſtruck with a ſingle blow, "by which the whole bulk, however vaſt, will "for a long time expand, and contract itſelf in "infinite elipſes. And when the attrition above

"deſcribed

" described is produced, with what force and velo-
" city are all the particles of the rubbed body
" compreſſed, ſhaken, and looſened to their very
" intimate ſubſtance *?" And in proportion to the ſwiftneſs of this vibration, and the violence of the attrition and friction, will be the heat that is produced.

A piece of iron that would ſuſtain the preſſure of any weight, however large, without being warmed, may be made quite hot by the blow of a hammer; and even ſoft and un-elaſtic bodies may be warmed by precuſſion, provided the velocity with which their parts are made to give way to the blow is ſufficiently rapid. If a leaden bullet be laid upon an anvil, or on any other hard body, and in that ſituation be ſtruck with the ſmart blow of a hammer, it will be found to be much heated; but the ſame bullet, in the ſame ſituation, may be much more flattened by preſſure, or by the ſtroke of a very heavy body moving with a ſmall velocity, without being ſenſibly warmed.

To generate heat therefore the action of the powder on the inſide of the piece muſt not only be ſufficient to *ſtrain the metal*, and produce a motion in its parts, but this effect muſt be *extremely rapid*; and the heat will be much augmented, if the exertion of the force and the duration of its action are momentaneous; for in that caſe, the fibres of the metal (if I may uſe the expreſſion)

* Vide Shaw's tranſlation of Boerhave's Chemiſtry, vol. i. p. 249.

that

that are violently ſtretched, will return with their full force and velocity, and the ſwift vibratory motion and attrition before deſcribed will be produced.

The heat generated in a piece by firing is therefore, as the force by which the particles of the metal are ſtrained and compreſſed—the ſuddenneſs with which this force is exerted—and the ſhortneſs of the time of its action; that is to ſay, as the ſtrength of the powder and the quantity of the charge—the quickneſs of its inflammation—and the velocity with which the generated fluid makes its eſcape.

Now the effort of any given charge of powder upon the gun is very nearly the ſame, whether it be fired with a bullet, or without a bullet; but the velocity with which the generated elaſtic fluid makes its eſcape is much greater when the powder is fired alone, than when it is made to impel one or more bullets; the *heat* ought therefore to be greater in the former caſe than in the latter, as I found it to be by experiment.

But to make this matter ſtill plainer, we will ſuppoſe any given quantity of powder to be confined in a ſpace that is juſt capable of containing it, and that, in this ſituation, it is by any means ſet on fire. Let us ſuppoſe this ſpace to be the chamber of a piece of ordnance of any kind, and that a bullet, or any other ſolid body, is ſo firmly fixed in the bore, immediately upon the charge, that the whole effort of the powder ſhall not be able to remove it. As the powder goes on to be inflamed,

inflamed, and the elaſtic fluid is generated, the preſſure upon the inſide of the chamber will be increaſed, till at length all the powder being burnt, the ſtrain upon the metal will be at its greateſt height, and in this ſituation things will remain, the cohefion or elaſticity of the particles of metal counterbalancing the preſſure of the fluid.

Under theſe circumſtances very little heat would be generated; for the continued effort of the elaſtic fluid would approach to the nature of the preſſure of a weight; and that *concuſſion*, *vibration*, and *friction*, among the particles of the metal, which, in the colliſion of elaſtic bodies is the cauſe of the heat that is produced, would ſcarcely take effect.

But inſtead of being firmly fixed in its place, let the bullet now be moveable, but let it give way with great difficulty, and by ſlow degrees. In this caſe, the elaſtic fluid will be generated as before, and will exert its whole force upon the chamber of the piece; but as the bullet gives way to the preſſure, and moves on in the bore, the fluid will expand itſelf and grow weaker, and the particles of the metal will gradually return to their former ſituations; but the velocity with which the metal reſtores itſelf, being but ſmall, the *vibration* that remains in the metal, after the elaſtic fluid has made its eſcape, will be very languid, as will be the *heat* that is generated by it.

But if inſtead of giving way with ſo much difficulty, the bullet be much lighter, ſo as to afford but little reſiſtance to the elaſtic fluid in making

its

its efcape, or if the powder be fired without any bullet at all; then there being little or nothing to oppofe the flame in its paffage through the bore, it will expand itfelf with an amazing velocity, and its action upon the gun will ceafe almoft in an inftant—the ftrained metal will reftore itfelf with a very rapid motion—and a fharp vibration will enfue, by which the piece will be *much heated.*

Of the effect of ramming the powder in the chamber of the piece.

The charge, confifting of 218 grains of powder, being put gently into the bore of the piece in a cartridge of very fine paper, without being rammed, the velocity of the bullets at a mean of the 40th, 41ft, 42d, and 47th experiments, was at the rate of 1225 feet in a fecond; but in the 68th, 69th, and 70th experiments, when the fame quantity of powder was rammed down with five or fix hard ftrokes of the ram-rod, the mean velocity was 1329 feet in a fecond. Now the total force or preffure exerted by the charge upon the bullet is as the fquare of its velocity, and $\overline{1329}^2$ is to $\overline{1225}^2$ as 1, 1776 is to 1; or nearly as 6 is to 5; and in that proportion was the force of the given charge of powder increafed by being rammed.

In the 71ft experiment the powder was alfo rammed, but the vent, inftead of being at the bottom of the bore, was at 1, 3, and the velocity of the bullet was very confiderably diminifhed, being only at the rate of 1080 feet in a fecond, inftead of 1276

feet

feet in a ſecond, which was the mean velocity with this charge, and with the vent in this ſituation when the powder was rammed. See the experiments N° 43, 44, 45, and 46.

When, inſtead of ramming the powder, or preſſing it gently together in the bore, it is put into a ſpace larger than it is capable of filling, the force of the charge is thereby very ſenſibly leſſened, as Mr. ROBINS and others have found by repeated trials. In my 30th experiment, the charge, conſiſting of no more than 165 grains of powder, was made to occupy 3,2 inches of the bore, inſtead of 1,45 inches, which ſpace it juſt filled when it was gently puſhed into its place without being rammed; the conſequence was, the velocity of the bullet, inſtead of being 1100 feet in a ſecond, or upwards, was only at the rate of 914 feet in a ſecond, and the recoil was leſſened in nearly the ſame proportion.

And from hence we may draw this practical inference, that the powder, with which a piece of ordnance, or a fire arm is charged, ought always to be preſſed together in the bore; and if it be rammed to a certain degree, the velocity of the bullet will be ſtill farther increaſed. It is well known, that the recoil of a muſket is greater when its charge is rammed, than when it is not; and there cannot be a ſtronger proof that ramming increaſes the force of the powder.

Of the relation of the velocities of bullets to the charges of powder by which they are impelled.

It appears by all the experiments that have hitherto been made upon the initial velocities of

bullets, that when the weights and dimenſions of the bullets are the ſame, and they are diſcharged from the ſame piece by different quantities of powder, the velocities are nearly in the ſub-duplicate ratio of the weights of the charges.

The following table will ſhew how accurately this law obtained in the foregoing experiments.

Charges.	Velocities Computed.	Actual.	Difference.	N° of exp.
437½	1764	1764	———	3
330	1533	1594	+ 61	2
310	1486	1459	− 27	1
290	1436	1436	0	7
218	1232	1225	− 7	4
208	1216	1256	+ 40	3
165	1083	1087	+ 4	2
145	1018	1040	+ 22	2
104	860	757	−103	2

The computed velocities, as they are ſet down in this table, were determined from the ratio of the ſquare root of 437½ — (the weight, in grains, of the largeſt charge of powder) — to the mean velocity of the bullet with that charge and the vent at 0; *viz.* 1764 feet in a ſecond, and the ſquare root of the other charges expreſſed in grains. And the *actual* velocities are means of all experiments that were made under ſimilar circumſtances, with the given charges.

The fourth column ſhews the difference of the computed and actual velocities, or the number of feet in a ſecond by which the actual velocity exceeds or falls ſhort of the computed: and in the

fifth column is ſet down in the number of experiments with each charge, from the mean of which the actual velocity was determined.

The agreement of the computed and actual velocities will appear more ſtriking, if we take the ſum and difference of thoſe velocities with all the charges except the firſt: thus,

Sum of the velocities, — 1764.

Computed.	Actual.	Difference.	N° of exp.
9864	9854	−10	23

So that it appears, that the difference, or the actual velocity, was ſmaller than the computed by $\frac{1}{985}$ part only, at a mean of 23 experiments.

But as by far the greater number of the experiments were made with the following charges, *viz.* 290, 218, 208, 165, and 145 grains of powder, let us take the ſum and difference of the computed and actual velocities of thoſe charges: thus,

Sum of the velocities

Computed.	Actual.	Difference.	N° of exp.
5985	6044	+59	18

Here the agreement of the theory with the experiments is ſo very remarkable, that we muſt ſuppoſe it was ſome meaſure accidental; for the difference of the velocities in repeating the ſame experiment is in general much greater than the difference of the computed and actual velocities in this inſtance; but I think, we may fairly conclude, from the reſult of all theſe trials, that the velocities of like *muſket* bullets,

when

when they are discharged from the same piece by different quantities of the same kind of powder, are very nearly in the sub-duplicate ratio of the weights of the charges. Whether this law will hold good when applied to cannon balls, and bomb shells of large dimensions, I dare not at present take upon me to decide; but, for several reasons that might be mentioned, I am rather of opinion, that it will not; at least not with that degree of accuracy which obtained in these experiments.

Of the effect of placing the vent in different parts of the charge.

There have been two opinions with respect to the manner in which gun-powder takes fire. Mr. ROBINS supposes that the progress of its inflammation is so extremely rapid, "that all the powder "of the charge is fired and converted into an elastic "fluid, before the bullet is sensibly moved from its "place;" while others have been of opinion, that the progress of the inflammation is much slower, and that the charge is seldom or never completely inflamed before the bullet is out of the gun.

The large quantities of powder that are frequently blown out of fire arms un-inflamed, seem to favour the opinion of the advocates for the gradual inflammation; but Mr. ROBINS endeavours to account for that circumstance upon different principles; and supports his opinion by shewing that every increase of the charge, within the limits of practice, produces a proportional increase of the velocity of the bullet;

and that when the powder is confined by a great additional weight, by firing two or more bullets at a time, instead of one, the velocity is not sensibly greater than it ought to be according to his theory.

If this were a question merely speculative, it might not be worth while to spend much time in the discussion of it; but as it is a matter upon the knowledge of which depends the determination of many important points respecting artillery, and from which many useful improvements may be derived, too much pains cannot be taken to come at the truth. Till the manner in which powder takes fire, and the velocity with which the inflammation is propagated, are known, nothing can with certainty be determined with respect to the best form for the chambers of pieces of ordnance, or the most advantageous situation for the vent; nor can the force of powder, or the strength that is required in different parts of the gun, be ascertained with any degree of precision.

As it would be easy to determine the best situation for the vent, from the velocity of the inflammation of powder being known, so on the other hand, I had hopes of being able to come at that velocity by determining the effect of placing the vent in different parts of the charge; for which purpose the following experiments were made.

A table

A table of experiments, ſhewing the effect of placing the vent in different parts of the charge.

Weight of the charge of powder.	Space occupied by the powder.	Vent from the bottom of the bore.	Velocity of the bullet at a medium.	Recoil measured upon the ribbon at a medium.	Number of experiments.
Grains.	Inches.	Inches.	Ft. in a ſec.	Inches.	
165	1,45	0	1087	14,465	2
. . .	. . .	1,32	1082	14,31	3
218	1,9	0	1225	17,93	4
. . .	. . .	1,3	1276	18,34	4
290	2,6	0	1427	22,626	3
. . .	. . .	1,3	1493	23,34	3
. . .	. . .	2,6	1460	23,286	4
. . .	. . .	0	1444	23,135	4
. . .	. . .	2,6	1413	24,5	3
310	2,7	0	——	24,69	1
. . .	. . .	1,32	——	24,95	1
. . .	. . .	2,65	——	24,9	1
330	2,9	0	1594	26,075	2
. . .	. . .	1,3	1625	26,4	2
. . .	. . .	2,6	1525	25,3	2
437½	3,9	0	1764	33,3	3
. . .	. . .	2,6	1751	32,866	3

By the foregoing experiments it appears, that the difference in the force of any given charge of powder which ariſes from the particular ſituation of the vent is extremely ſmall.

With 165 grains of powder, and the vent at 0, the velocity of the bullet at a mean of two experiments (*viz.* the 20th and 21ſt) was 1087 feet in a ſecond; and with the ſame charge, and the vent at 1,32 inches, the velocity at a mean of the 22d, 23d, and 24th experiments, was 1082 feet in a

ſecond; the difference, (equal to five feet in a ſecond,) is leſs than what frequently occurred in a repetition of the ſame experiment.

With 218 grains of powder, and the vent at 0, the velocity, at a mean in the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; and with the ſame charge, and the vent at 1,3, the velocity was 1276 feet in a ſecond, at a mean of four experiments, *viz.* the 43d, 44th, 45th, and 46th.

In the firſt ſet of experiments, with 290 grains of powder, the velocities were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
1414	1476	1502
1455	1520	1450
1412.	1483	1433
		1454
3)4281	3)4479	4)5839
Means 1427	1493	1460

See the experiments from N° 48. to N° 57. inclusive.

In the ſecond ſet the velocities were,

Vent at 0.	Vent at 2,6.
1419	1438
1460	1423
1462	1378
1436.	
4)5777	3)4239
Means 1444	1413

See the experiments from N° 109. to N° 115. inclusive.

And

And taking the means of all the velocities in both ſets in each poſition of the vent it will be,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Mean velocity	1436	1493	1437

The mean recoils in theſe experiments were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,88	23,34	23,61

In the experiments with 310 grains of powder the velocities of the bullets were not determined with ſufficient accuracy to be depended on; but the recoils, which were meaſured with great nicety, were as follows, *viz.*

Vent at 0.	Vent at 1,3.	Vent at 2,6.
24,69	24,95	24,9

With 330 grains of powder the mean velocities and recoils were,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Velocities	1594	1625	1525
Recoils	26,075	26,4	25,3

In the experiments with 437½ grains (an ounce avoirdupois) of powder the velocities and recoils were,

Vent at 0.		Vent at 2,6.	
Velocity.	Recoil.	Velocity.	Recoil.
1738	33,	1707	32,5
1824	33,3	1757	33,2
1728	33,6	1789	32,9
3)5291	3)99,9	3)5253	3)98,6
Ms. 1764	33,3	1751	32,866

From the result of these experiments it appears, that the effect of placing the vent in different positions with respect to the bottom of the chamber, is different, in different charges; thus, with 165 grains of powder the velocity of the bullet was rather diminished by removing the vent from 0, or the bottom of the bore to 1.32; but with 218 grains of powder the velocity was a little increased, as was also the recoil. With 290 grains of powder the velocity was greatest when the powder was lighted at the vent 1,3 which was near the middle of the charge, and rather greater when it was lighted at the top, or immediately behind the bullet, than when it was lighted at the bottom. And by the recoil it would seem, that the velocities of the bullets varied nearly in the same manner when the charge consisted of 310 grains of powder.

With 330 grains of powder, both the velocity and the recoil were greater when the powder was lighted at the middle of the charge, than when it was lighted at the bottom; but they were least of all when it was lighted near the top. And when *an ounce* of powder was made use of for the charge, its force was greatest when it was lighted at the bottom. But the difference in the force exerted by the powder which arose from the particular position of the vent was in all cases so inconsiderable (being, as I have before observed, less than what frequently occurred in repeating the same experiment) that no conclusion can be drawn from the experiments, except only this, that any given charge of powder exerts nearly the same force, whatever is the position of the vent.

And

And hence the following practical inference naturally occurs, *viz.* that in the conſtruction of fire-arms no regard need be had to any ſuppoſed advantages that gun-ſmiths and others have hitherto imagined were to be derived from particular ſituations for the vent, ſuch as diminiſhing the recoil, increaſing the force of the charge, &c.; but the vent may be, indifferently, in any part of the chamber where it will beſt anſwer upon other accounts: and there is little doubt but the ſame thing will hold good in great guns, and all kinds of heavy artillery.

Almoſt every workman who is at all curious in fire-arms has a particular fancy with regard to the beſt form for the bottom of the chamber, and the proper poſition of the vent. They in general agree, that the vent ſhould be as low, or far back as poſſible, in order, as they pretend, to leſſen the recoil; but no two of them make it exactly in the ſame manner. Some make the bottom of the chamber flat, and bring the vent out even with the end of the breech-pin. Others make the vent ſlanting through the breech-pin, in ſuch a manner as to enter the bore juſt in its axis. Others again make the bottom of the chamber conical; and there are thoſe who make a little cylindric cavity in the breech-pin, of about two-tenths of an inch in diameter, and near half an inch in length, coinciding with the axis of the bore, and bring out the vent even with the bottom of this little cavity.

The objection to the firſt method is, the vent is apt to be ſtopped up by the foul matter that adheres to the

the piece after firing, and which is apt to accumulate, especially in damp weather. The same inconvenience, in a still greater degree, attends the other methods, with the addition of another, arising from the increased length of the vent; for the vent being longer, it is not only more liable to be obstructed, but it takes a longer time for the flame to pass through it into the chamber, in consequence of which the piece is slower in going off, or, as sportsmen term it, is apt to hang fire.

The form I would recommend for the bottom of the bore is that of a hemisphere; and the vent should be brought out directly through the side of the barrel, in a line perpendicular to its axis, and pointing to the center of the hemispheric concavity of the chamber.

In this case the vent would be the shortest possible; it would be the least liable to be obstructed, and the piece would be more easily cleaned, than if the bottom of the bore was of any other form. All these advantages, and several others not less important, would probably be gained by making the bottom of the bore and vent of great guns in the same manner.

A new method of determining the velocities of bullets.

From the equality of *action* and *re-action*, it appears, that the *momentum* of a gun must be precisely equal to the momentum of its charge; or, that the weight of the gun, multiplied into the velocity of its recoil, must be in all cases just equal to the weights of the bullet, and of the powder (or the elastic fluid that is generated from it) multiplied in-

to

to their reſpective velocities :—for every particle of matter, whether ſolid or fluid, that iſſues out of the mouth of a piece, muſt be impelled by the action of ſome power, which power muſt *re-act*, with equal force, againſt the bottom of the bore.

Even the fine inviſible elaſtic fluid that is generated from the powder in its inflammation cannot put itſelf in motion without re-acting againſt the gun at the ſame time. Thus we ſee pieces, when they are fired with powder alone, recoil, as well as when their charges are made to impel a weight of ſhot, though the recoil is not in the ſame degree in both caſes.

It is eaſy to determine the velocity of the recoil in any given caſe, by ſuſpending the gun in an horizontal poſition by two pendulous rods, and meaſuring the arc of its aſcent, by means of a ribbon, according to the method already deſcribed ; and this will give the momentum of the gun, (its weight being known,) and conſequently the momentum of its charge. But in order to determine the velocity of the bullet from the recoil, it will be neceſſary to find out how much the weight and velocity of the elaſtic fluid contributes to produce that recoil.

That part of the recoil which ariſes from the expanſion of this fluid is always very nearly the ſame, whether the powder is fired alone, or whether the charge is made to impel one or more bullets, as I have found by a great variety of experiments.

If therefore, a gun, ſuſpended according to the method preſcribed, be fired with any given charge of powder, but without any bullet or wad, and the recoil be obſerved, and if the ſame piece be afterwards

fired

fired with the ſame quantity of powder, and a bullet of a known weight, the exceſs of the velocity of the recoil in the latter caſe, over that in the former, will be proportional to the velocity of the bullet; for the difference of theſe velocities, multiplied into the weight of the gun, will be equal to the weight of the bullet multiplied into its velocity.

Thus if W be put equal to the weight of the gun,

U = the velocity of its recoil, when it is fired with any given charge of powder, without any bullet,

V = the velocity of the recoil, when the ſame charge is made to impel a bullet,

B = the weight of the bullet, and

v = its velocity,

It will be $v = \frac{\overline{V-U}+W}{B}$

Let us ſee how this method of determining the velocities of bullets will anſwer in practice.

In the 94th experiment, the recoil, with 165 grains of powder, without a bullet, was 5,5 inches, and in the 95th experiment, with the ſame charge, the recoil was 5,6 inches. The mean is 5,55 inches; and the length of the rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil (=U) anſwering to 5,55 inches meaſured upon the ribbon, is that of 1,1358 feet in a ſecond.

In five experiments with the ſame charge of powder, and a bullet weighing 580 grains, the recoil was as follows, *viz.*

The

The 20th experiment			14,73 inches
21ſt	-	-	14,2
22d	-	-	14,8
23d	-	-	14,58
24th	-	-	14,68

5)73, (=14,6 inches at a mean.

And the velocity of the recoil (=V) anſwering to this length (14,6 inches) is that of 2,9880 feet in a ſecond: conſequently V−U, or 2,9880 −1,1358 is equal to 1,8522 feet in a ſecond.

But as the velocities of the recoil are known to be as the chords of the arcs through which the barrel aſcends, it is not neceſſary, in order to determine the velocity of the bullet, to compute the velocities V and U; but the quantity $\overline{V-U}$, or the difference of the velocities of the recoil when the given charge is fired with, and without a bullet, may be computed from the value of the difference of the chords, by one operation. Thus the velocity anſwering to the chord 9,05 = 14,6 − 5,55 is that of 1,8522 feet in a ſecond, which is juſt equal to $\overline{V-U}$, as was before found.

The weight of the barrel, together with its carriage, was 47¼ pounds, to which three quarters of a pound is to be added on account of the weight of the rods by which it was ſuſpended, which makes W = 48 pounds, or 336,000 grains; and the weight of the bullet was 580 grains. B is therefore to W as 580 is to 336,000, that is, as

1 is

1 is 579,31, very nearly; and $v\left(=\frac{\overline{V-U}\times W}{B}\right)$ is equal to $\overline{V-U}+579{,}31$.

The value of $\overline{V-U}$ anſwering to the experiments before mentioned was found to be 1,8522; conſequently the velocity of the bullets ($=v$) was $1{,}8522\times 579{,}31=1073$ feet in a ſecond, which is extremely near 1083 feet in a ſecond, the mean of the velocities, as they were determined by the pendulum.

But the computation for determining the velocity of a bullet, upon theſe principles, may be rendered ſtill more ſimple and eaſy in practice; for the velocities of the recoil being as the chords meaſured upon the ribbon, if

c be put equal to the end of the chord of the recoil, expreſſed in Engliſh inches, when the piece is fired with powder only,

and C = the chord when a bullet is diſcharged by the ſame charge,

then $C-c$ will be as $V-U$; and conſequently as $\frac{\overline{V-U}\times W}{B}$, which meaſures the velocity of the bullet; the ratio of W to B remaining the ſame.

If therefore we ſuppoſe a caſe in which $C-c$ is equal to one inch, and the velocity of the bullet be computed from that chord, the velocity in any other caſe, in which $C-c$ is greater or leſs than one inch, will be found by multiplying the difference of the chords C and c by the velocity that anſwers to a difference of one inch.

The

The length of the parallel rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil anſwering to $C - c = 1$ inch, meaſured upon the ribbon, is 0,204655 parts of a foot in a ſecond; and this is alſo, in this caſe, the value of $V - U$; the velocity of the bullet is therefore $v = 0{,}204655 \times 579{,}31 = 118{,}35$ feet in a ſecond.

Conſequently the velocity of the bullet, expreſſed in feet *per* ſecond, may in all caſes be found by multiplying the difference of the chords C, and *c*, by 118,35 the weight of the barrel, the length of the rods by which it is ſuſpended, and the weight of the bullet remaining the ſame; and this, whatever the charge of powder may be that is made uſe of, and however it may differ in ſtrength or goodneſs.

According to this rule, the velocities of the bullets in the following experiments have been computed, from the recoil; and by comparing them with the velocities ſhewn by the pendulum, we ſhall be enabled to judge of the accuracy of this new method of determining the velocities of bullets.

In the 76th and 77th experiments, when the piece was fired with 145 grains of powder and a bullet, the recoil was 13,25 and 13,15, or 13,2 at a mean; and with the ſame charge of powder, without a bullet, the recoil was 4,5 and 4,3, or 4,4 at a mean (ſee the 84th and 93d experiments).

$C - c$ is therefore $13{,}2 - 4{,}4 = 8{,}8$ inches, and the velocity of the bullets $= 8{,}8 \times 118{,}35 = 1045$ feet in a ſecond. The mean of the velocities as

they

they were determined by the pendulum is that 1040 feet in a ſecond. In the 104th and 105th experiments the recoil was 12,92 and 13,28, and the velocity computed from the mean of thoſe chords is 1030 feet in a ſecond; but the velocity ſhewn by the pendulum was no more than about 900 feet in a ſecond. As the recoil was ſo nearly equal to what it was in the 76th and 77th experiments before mentioned, when the velocities ſhewn by the recoil and by the pendulum were almoſt exactly the ſame, I am inclined to believe, that there muſt have been ſome miſtake in determining the velocities by the pendulum, in theſe laſt experiments, and that the velocity ſhewn by the recoil is moſt to be depended on.

With 290 grains, or half the weight of the bullet in powder, in the 48th, 49th, and 50th experiments, the recoil was 22,58, 22,92, and 22,38; and the recoil, with the ſame charge of powder, without a bullet, at a mean of the 60th and 99th experiments, was 10,66. The mean of the velocities of the bullets, computed from the recoil, is therefore 1416 feet in a ſecond, and the velocity ſhewn by the pendulum was 1427 feet in a ſecond; the difference is not conſiderable. The mean of the velocities in the 109th, 110th, 111th, 112th experiments is by the recoil 1464, and by the pendulum 1444 feet in a ſecond.

With 330 grains of powder the velocities of the bullets appear to have been as follows, *viz.*

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil	1543	1620	1610
By the pendulum	1594	1625	1528

See the 62d, 63d, 64th, 65th, 66th, 67th, and 17th experiments.

The *uniformity of the recoil* was in all cafes very remarkable. Thus, in the firſt ſet of experiments with 290 grains of powder (from the 48th to the 57th experiment inclufive), the recoil, was

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,58	23,21	23,06
22,92	23,76	23,26
22,38	23,06	23,26
		23,56
3)67,88	3)70,03	4)93,14
Means = 22,626	23,343	and 23,285

If now we take a mean of the 60th and 99th experiments, and call the recoil, without a bullet, 10,66, as before, the velocities will turn out,

	Vent at 0.	Vent at 1,3	Vent at 2,6.
By the recoil - - -	1416	1501	1494
And by the pendulum they were	1427	1493	1494
The difference is only -	− 11	+ 8	+ 34

The recoil was equally regular in the 117th and five ſucceeding experiments, when the charge was no leſs than 437½ grs. = 1 ounce avordupois in powder; and the velocities of the bullets determined from the recoil are very nearly the ſame as they were ſhewn by the pendulum. Thus, in the

117th, 118th, and 119th experiments the mean recoil was 33,3: and in the 120th, 121ſt and 122d experiments it was 32,866. And if the recoil without a bullet be called 17,9, as it was determined by the 123d experiment, which was made immediately after the experiments before mentioned, then will the velocities be

	Vent at 0.	Vent at 2,6.
By the recoil - - -	1822	1771
And by the pendulum they were	1764	1751

The difference is only - + 58 and + 20 feet in a ſecond, which is leſs than what frequently occurs in repeating the ſame experiment.

In the 11th, 12th, 13th and 14th experiments, when the piece was fired with 310 grains of powder, and a bullet, the recoil was 24,69. 24,95, 24,9, and 24,9: and in the 15th, 16th, 18th, and 19th experiments with 330 grains of powder, the recoil was 26,2, 26,2, 26,3, and 26,4. The regularity of theſe numbers is very ſtriking; and though we cannot compare the velocities of the bullets determined by the two methods, as we have done in other caſes (as there are reaſons to believe that the velocities, as they are ſet down in the tables, are not much to be depended on, and as the recoil, with the given charge of 310 grains of powder, without a bullet, is not known), yet the regularity of the recoil in theſe experiments affords good grounds to conclude, that

the

the method of determining the velocities of bullets founded upon it muſt be very accurate.

But of all the experiments thoſe numbered from 84 to 92, incluſive, afford the ſtrongeſt proof of the accuracy of this method. In thoſe, every poſſible precaution was taken to prevent errors ariſing from adventitious circumſtances, and the weights of the bullets, and their velocities, were ſo various, that the uniform agreement of the two methods of determining the velocities, in thoſe trials, amounts almoſt to a demonſtration of the truth of the principles upon which this new method is founded.

By the following table the reſult of theſe experiments may be ſeen at one view.

The experiments.	Weight of the bullets.	The barrel heavier than the bullet.	The recoil.	Velocity of the bullet. By the recoil	Velocity of the bullet. By the pendulum	Difference.
	Grs.	$\frac{W}{B}$ =	C =	v =	v =	
84th & 93d	——	——— (c=4,4)		——	——	——
85th	90	3733,3	7,16	2109	1763	+346
86th	251	1338,6	9,62	1430	1317	+113
87th	354	949,15	11,03	1288	1136	+152
88th	600	560,	15,22	1240	1229	+ 11
89th	603	557,22	15,13	1224	1229	+ 5
90th	1184	283,78	21,92	1017	978	− 39
91ſt	1754	191,56	27,18	893	916	− 23
92d	2352	141,86	32,25	812	833	− 21

The charge of powder conſiſted of 145 grains in weight in each experiment.

In order to ſhew, in a more ſtriking manner, the reſult of theſe experiments, and the compariſon of the two methods of aſcertaining the velocities of bullets, I have drawn the fig. 16. where the numbers that are marked upon the line A B, are taken from A towards B, in proportion to the weights of the bullets; while the lines drawn from thoſe numbers perpendicular from AB (as *w*, *v*, for inſtance, at the number 2352) and ending at the curve *c*, *d*, expreſs their velocities, as ſhewn by the pendulum. The continuation of thoſe lines on the oppoſite ſide of the line AB ſhew the recoil, and alſo the velocities of the bullets as determined from it; thus *w*, *r*, and the (dotted) lines parellel to it, which end at the line *g*, *f*, expreſs the recoil; and the portion of each of thoſe lines that is comprehended between the line AB and the curve *m*, *n*, (as *w*, *u*) is as the velocity of the bullet in the ſeveral experiments. The line *A*, *e*, denotes the weight of the charge of powder; and the line A, *m*, the velocity with which the elaſtic fluid eſcapes out of the piece, when the powder is fired without any bullet.

Upon an inſpection of this figure, as well as from an examination of the foregoing table, it appears, that the velocities determined by the two methods agree with great nicety in all the experiments after the 87th; but in the 87th experiment and alſo in the 86th, but particularly in the 85th, the difference in the reſult of theſe different methods is very conſiderable: and it is remarkable, that in thoſe experiments where they diſagree moſt,

the

the velocities of the bullets, as determined by the pendulum, are extremely irregular; while, on the other hand, the gradual increaſe of the recoil as the bullets were heavier, and the great regularity of the correſponding velocities, affords good grounds to conclude, that this diſagreement is not owing to any inaccuracy in the new method of aſcertaining the velocities, but to ſome other cauſe, which remains to be inveſtigated.

But before we proceed in this inquiry, let us ſeparate the five laſt experiments in the foregoing table; and ſumming up the velocities determined by the two methods, we ſhall ſee by their difference how thoſe methods agreed, upon the whole, in this inſtance.

Experiments.	Weight of the bullets.	Velocity. By the recoil.	Velocity. By the pendulum.	Difference
	Grs.			
88th	600	1240	1229	+ 11
89th	603	1224	1229	− 5
90th	1184	1017	978	+ 39
91ſt	1754	893	916	− 23
92d	2352	812	833	− 21
Sums & diff. of the velocities		5186	5185	+ 1

Here the difference in the reſult of the two methods does not amount to $\frac{1}{5000}$th part of the whole velocity; but I lay no ſtreſs upon this extraordinary argument. I am ſenſible that it muſt in ſome degree have been accidental; but as the difference in the velocities, computed by theſe different methods,

was in no inſtance conſiderable, not being in any caſe ſo great as what frequently occurred in the moſt careful repetition of the ſame experiment, and as the velocities, as determined by the recoil, were much more regular than thoſe ſhewn by the pendulum, as appears by comparing the curves *g*, *f*, and *m*, *n*, (fig. 16.) with the crooked line *c*, *d*, I think we may fairly conclude, that this new method may with ſafety be relied on in practice.

The greateſt difference in the velocities, as aſcertained by the two methods, appears, in the inſtance of the 85th experiment, where the velocity, determined from the recoil, exceeds that ſhewn by the pendulum, by 346 feet in a ſecond, the former velocity being that of 2109 feet in a ſecond, the latter only 1763 feet in a ſecond; and in the two ſucceeding experiments, the velocities ſhewn by the pendulum are likewiſe deficient, though not in ſo great a degree.

This apparent deficiency remains now to be accounted for; and, firſt, it cannot be ſuppoſed, that it aroſe from any imperfection in Mr. Robins's method of determining the velocities of bullets; for that method is founded upon ſuch principles as leave no room to doubt of its accuracy; and the practical errors that occur in making the experiments, and which cannot be entirely prevented, or exactly compenſated, are in general ſo ſmall, that the difference of the velocities in queſtion cannot be attributed to them. It is true, the effect of thoſe errors is more likely to appear in experiments made under ſuch circumſtances as thoſe under

which

which the experiments we are now ſpeaking of were made, than in any other caſe; for the bullets being very light, the arc of the aſcent of the pendulum was but ſmall, and a ſmall miſtake in meaſuring the chord upon the ribbon would have produced a very conſiderable error in computing the velocity of the bullet; thus, a difference of one tenth of an inch, more or leſs, upon the ribbon in the 85th experiment, would have made a difference in the velocity of more than 120 feet in a ſecond. But independent of the pains that were taken to prevent miſtakes, the ſtriking agreement of the velocities determined by the two methods in the experiments which immediately follow, as alſo in all other caſes where they could be compared, affords abundant reaſon to conclude, that the errors ariſing from thoſe cauſes were in no inſtance very conſiderable.

But if both methods of aſcertaining the velocities of bullets are to be relied on, then the difference of the velocities, as determined by them, in theſe experiments, can only be accounted for by ſuppoſing that it aroſe from their having been diminiſhed by the reſiſtance of the air in the paſſage of the bullets from the mouth of the piece to the pendulum; and this ſuſpicion will be much ſtrengthened when we conſider how great the reſiſtance is, that the air oppoſes to bodies that move very ſwiftly in it, and that the bullets in theſe experiments were not only projected with great velocities, but were alſo very light, and conſequently

ſequently more liable to be retarded by the reſiſtance on that account.

To put the matter beyond all doubt, let us ſee what the reſiſtance was that theſe bullets met with, and how much their velocities were diminiſhed by it. The weight of the bullet (in the 85th experiment) was 90 grains; its diameter was 0,78 of an inch, and it was projected with a velocity of 2109 feet in a ſecond.

If now a computation be made according to the method laid down by Sir ISAAC NEWTON for compreſſed fluids, it will be found, that the reſiſtance to this bullet was not leſs than $8\frac{1}{2}$ lbs. avordupois, which is ſomething more than 660 times its weight. But Mr. ROBINS has ſhewn, by experiment, that the reſiſtance of the air to bodies moving in it with very great velocity is near three times greater than Sir ISAAC has determined it, and as the velocity with which this bullet was impelled is conſiderably greater than any in Mr. ROBINS's experiments, it is highly probable, that the reſiſtance in this inſtance was at leaſt 2000 times greater than the weight of the bullet.

The diſtance from the mouth of the piece to the pendulum, as we have before obſerved, was 12 feet; but, as there is reaſon to think, that the blaſt of the powder, which always follows the bullet, continues to act upon it for ſome ſenſible portion of time, after it is out of the bore, and by urging it on, counter-balances, or at leaſt counteracts in a great meaſure the reſiſtance of the air, we will ſuppoſe, that the reſiſtance does not begin,

or

or rather, that the motion of the bullet does not begin to be retarded, till it has got to the diſtance of two feet from the muzzle. The diſtance, therefore, between the barrel and the pendulum, inſtead of 12 feet, is to be eſtimated at 10 feet; and as the bullet took up about $\frac{1}{192}$ part of a ſecond in running over that ſpace, it muſt, in that time, have loſt a velocity of about 335 feet in a ſecond, as will appear upon making the computation, and this will very exactly account for the apparent diminution of the velocity in the experiment; for the difference of the velocities, as determined by the recoil and by the pendulum, $= 2109 - 1763 = 346$ feet in a ſecond, is extremely near 335 feet in a ſecond, the diminution of the velocity by the reſiſtance as here determined.

If the diminution of the velocities of the bullets in the two ſubſequent experiments be computed in like manner, it will turn out in the 86th experiment $= 65$ feet in a ſecond, and in the 87th experiment $= 33$ feet in a ſecond; and making theſe corrections, the compariſon of the two methods of aſcertaining the velocities will ſtand thus:

	85th exp.	86th exp.	87th exp.
Velocities ſhewn by the pendulum,	1763	1317	1136
Add the diminution of the velocity by the reſiſtance of the air.	335	65	33
Making together - -	2098	1382	1169
Velocity by the recoil, -	2109	1430	1288
The difference =	+ 11	+ 48	+ 119

So

So that it appears, notwithſtanding theſe corrections, that the velocities in the 86th and 87th experiments, and particularly in the laſt, as they were determined by the pendulum, are ſtill conſiderably deficient. But the manifeſt irregularity of the velocities in thoſe inſtances, affords abundant reaſon to conclude, that it muſt have ariſen from ſome extraordinary accidental cauſe, and therefore, that little dependance is to be put upon the reſult of thoſe experiments. I cannot take upon me to determine poſitively, what the cauſe was which produced this irregularity; but I ſtrongly ſuſpect, that it aroſe from the breaking of the bullets in the barrel by the force of the exploſion; for theſe bullets, as has already been mentioned, were formed of lead, incloſing leſſer bullets of plaſter of Paris; and I well remember to have obſerved at the time, ſeveral ſmall fragments of the plaſter, which had fallen down by the ſide of the pendulum. I confeſs, I did not then pay much attention to this circumſtance, as I naturally concluded, that it aroſe from the breaking of the bullet in penetrating the target of the pendulum, and that the ſmall pieces of plaſter I ſaw upon the ground had fallen out of the hole by which the bullet entered. But if the bullets were not abſolutely broken in pieces in the firing, yet, if they were conſiderably bruiſed, and the plaſter, or a part of it, were ſeparated from the lead, ſuch a change in their form might produce a great increaſe of the reſiſtance, and even their initial velocities might be affected by it, for their form being changed from that of a globe, to ſome other figure,

they

they might not fit the bore, and a part of the force of the charge might be loſt by the windage.

That this actually happened in the 87th experiment ſeems very probable, as the velocity with which the bullet was projected, even as it was determined by the recoil, is conſiderably leſs, in proportion, in that experiment, than in either of thoſe that precede it, in that ſet, or in thoſe which follow it, as will appear upon inſpecting the curvature of the line *m*, *n*, fig. 16. But I forbear to inſiſt further upon this matter.

As I have made an allowance for the reſiſtance of the air in theſe experiments, it may be expected that I ſhould do it in all other caſes; but, I think, it will appear, upon enquiry, that the diminution of the velocities of the bullets on that account was in general ſo inconſiderable that it might ſafely be neglected: thus, for inſtance, in the experiments with an ounce of powder, when the veloctiy of the bullet was more than 1750 feet in a ſecond, the diminution turns out no more than about 25 or 30 feet in a ſecond, though we ſuppoſe the full reſiſtance to have begun ſo near as two feet from the mouth of the piece; and in all caſes where the velocities were leſs, the effect of the reſiſtance was leſs in a much greater proportion; and even in this inſtance there is reaſon to think, that the diminution of the velocity, as we have determined it, is too great; for the flame of gun-powder, expands with ſuch an amazing rapidity, that it is ſcarcely to be ſuppoſed but that it follows the bullet, and continues to act upon it, more than

two

two feet, or even four feet, from the gun, and when the velocity of the bullet is leſs, its action upon it muſt be ſenſible at a ſtill greater diſtance.

With 218 grains of powder the recoil appears to have been very uniform; but if the velocities of the bullets are determined from the recoil in the 40th and ſeven following experiments, when this charge was made uſe of, and from the recoil without a bullet in the 72d and 73d experiments, the velocities will turn out conſiderably too ſmall, as we ſhall ſee by making the computation.

	Vent at 0.		Vent at 1,3.
The recoil in the	40th exp. was 18,	and in the 43d exp. it was	18,3
	41ſt - 17,71	- 44th -	18,35
	42d - 17,91	- 45th -	18,35
	47th - 18,1	- 46th -	18,35
	4)71,72		4)73,35
	Means = 17,93	and	18,34

And in the 72d and 73d experiments the recoil, with the ſame charge without a bullet, was 8,72, and 8,47 = 8,595 at a medium, the velocities therefore turn out,

	Vent at 0.	Vent at 1,3.
By the recoil	1105 -	1153
inſtead of	1225 and	1276 as they were ſhewn by the pend.
The diff.	120 and	123 feet in a ſecond amounts to near one twelfth part of the whole velocity.

This difference is undoubtedly owing to the recoil without a bullet being taken too great; for it

is

is not only greater than it ought to be, in order that the velocities of the bullets may come out right; but it is conſiderably greater in proportion than the recoil with any other charge.

Thus, with 145 grains	of powder	the recoil was	4,4
with 165 grains	- -	it was	5,55
290 grains	- - -		10,66
330 grains	- - -		12,7
and with 437½ grains	- - -	it was	17,9

And if the recoil with 218 grains is determined from theſe numbers by interpolation, it comes out 7,5; and with that value for C the velocities of the bullets in the before mentioned experiments appear to be,

	Vent at 0.		Vent at 1,3.
	1243	and	1283 by the reeoil
which is extremely near	1225	and	1276 the velocities

ſhewn by the pendulum.

It is to be remembered, that the 72d and 73d experiments, from which we before determined the recoil with the given charge of powder, without a bullet, were not made upon the ſame day with the experiments before mentioned; and it is well known, that the force of powder is different upon different days. And it is worthy of remark, that in thoſe two experiments the ſtrength of government powder, appeared to be conſiderably the greateſt. I mention theſe circumſtances to ſhew the probability there is, that the recoil, in thoſe experiments, from ſome unknown cauſe, was greater than it ought to have been, or rather, than it would have been, had the experiments

periments been made at the ſame time when the experiments with the bullets were made; or at any other time under the ſame circumſtances.

As this method of determining the velocities of the bullets did not occur to me till after I had finiſhed the courſe of my experiments, and had taken down my apparatus, I have not had an opportunity of aſcertaining the recoil, with and without a bullet, with that degree of preciſion that I could wiſh. If I had thought of it ſooner, or if I had recollected that paſſage in Mr. ROBINS's new Principles of Gunnery, where he ſays, "The "part of the recoil, ariſing from the expanſion "of the powder alone, is found to be no greater "when it impels a leaden bullet before it, than "when the ſame quantity is fired without any "wad to confine it:" I ſay, if that paſſage had occurred to me before it had been too late, I certainly ſhould have taken ſome pains to have aſcertained the fact; but as it is, I think, enough has been done to ſhew, that there is the greateſt probability that the velocities of bullets may, in all caſes, be determined by the recoil with great accuracy; and I hope ſoon to have it in my power to put the matter out of all doubt, and to verify this new method by a courſe of concluſive experiments, which I am preparing for that purpoſe.

In the mean time I would juſt obſerve, that if this method ſhould be found to anſwer, when applied to muſket bullets, it cannot fail to anſwer equally well when it is applied to cannon balls,

and

and bomb ſhells of the largeſt dimenſions; and it is apprehended, that it will be much preferable to any method hitherto made public; not only as it may be applied indifferently to all kinds of military projectiles, and that with very little trouble and expence, in making the experiment; but alſo, becauſe by it the velocities with which bullets are *actually projected* are determined; whereas by the pendulum their velocities can only be aſcertained at ſome diſtance from the gun, and after they have loſt a part of their initial velocities by the reſiſtance of the air through which they are obliged to paſs to arrive at the pendulum.

At the trifling expence of ten or fifteen pounds, an apparatus might be conſtructed that would anſwer for making the experiments with all the different kinds of ordnance in the Britiſh ſervice. The advantages that might be derived from ſuch a ſet of experiments are too obvious to require being mentioned.

Of a very accurate method of proving gun-powder.

All the *eprouvettes*, or powder-triers in common uſe, are defective, in many reſpects. Neither the abſolute force of gun-powder can be determined by means of them, nor the comparative force of different kinds of it, but under circumſtances very different from thoſe in which the powder is made uſe of in ſervice.

As the force of powder ariſes from the action

of an elaſtic fluid that is generated from it in its inflammation, the quicker the charge takes fire, the more of this fluid will be generated in any given ſhort ſpace of time, and the greater of courſe will be its effect upon the bullet. But in the common method of proving gun-powder, the weight by which the powder is confined, is ſo great in proportion to the quantity of the charge, that there is time, quite ſufficient, for the charge to be all inflamed, even when the powder is of the floweſt compoſition, before the body to be put in motion can be ſenſibly removed from its place. The experiment, therefore, may ſhew which of two kinds of powder is the ſtrongeſt, when equal quantities of both are confined in equal ſpaces, and *completely inflamed;* but the degree of inflammability, which is a property eſſential to the goodneſs of the powder, cannot, by theſe means, be aſcertained.

Hence it appears, how powder may anſwer to the proof, ſuch as is commonly required, and may nevertheleſs turn out very indifferent when it comes to be uſed in ſervice. And this, I believe, frequently happens; at leaſt I know that complaints from officers, of the badneſs of our powder, are very common; and I would ſuppoſe that no powder is ever received by the Board of Ordnance but ſuch as has gone through the eſtabliſhed examination, and has anſwered to the uſual teſt of its being of the ſtandard degree of ſtrength.

But though the common powder-triers may ſhew powder to be better than it really is, they never can

can make it appear to be worſe than it is; it will therefore always be the intereſt of thoſe who manufacture that commodity to adhere to the old method of proving it. But the purchaſer will find his account in having it examined in a manner, by which its goodneſs may be aſcertained with greater preciſion.

The method I would recommend is as follows. A quantity of powder being provided, which, from any previous examination, or trial, is known to be of a proper degree of ſtrength to ſerve as a ſtandard for the proof of other powder, a given charge of it is to be fired, with a fit bullet, in a barrel ſuſpended by two pendulous rods, according to the method before deſcribed, and the recoil is to be carefully meaſured upon the ribbon; and this experiment being repeated three or four times, or oftener, if there ſhould be any conſiderable difference, in the recoil, the mean and the extremes of the chords may be marked upon the ribbon, by black lines drawn acroſs it, and the word *proof* may be written upon the middle line; or if the recoil be uniform (which it will be to a ſufficient degree of accuracy, if care is taken to make the experiments under the ſame circumſtances) then the *proof mark* is to be made in that part of the ribbon to which it was conſtantly drawn out by the recoil, in the different trials.

The recoil with a known charge of ſtandard-powder, being thus aſcertained, and marked upon the ribbon, let an equal quantity of any other powder (that is to be proved) be fired in the ſame

barrel, with a bullet of the ſame weight, and every other circumſtance alike, and if the ribbon is drawn out as far, or farther than the proof mark, the powder is as good, or better than the ſtandard; but if it falls ſhort of that diſtance, it is worſe than the ſtandard, and is to be rejected.

For the greater the velocity is, with which the bullet is impelled, the greater will be the recoil; and when the recoil is the ſame, the velocities of the bullets are equal, and the powder is of the ſame degree of ſtrength, if the quantity of the charge is the ſame. And if care is taken in proportioning the charge to the weight of the bullet, to come as near as poſſible to the medium proportion that obtains in practice, the determination of the goodneſs of gun-powder from the reſult of this experiment, cannot fail to hold good in actual ſervice.

Fig. 14. repreſents the propoſed apparatus, drawn to a ſcale of one foot to the inch. *a*, *b*, is the barrel ſuſpended by the pendulous rods *c*, *d*; and *r* is the ribbon for meaſuring the recoil.

The length of the bore is 30 inches, and its diameter is one inch, conſequently it is juſt 30 calibres in length, and will carry a leaden bullet of about 3 ounces.

The barrel may be made of gun-metal, or of caſt iron, as that is a cheaper commodity; but great care muſt be taken in boring it, to make the cylinder perfectly ſtrait and ſmooth, as well as to preſerve the proper dimenſions. Of whatever metal the barrel is made, it ought to weigh at

leaſt 50 lbs. in order that the velocity of the recoil may not be too great; and the rods by which it is ſuſpended ſhould be five feet in length. The vent may be about one-twentieth of an inch in diameter; and it ſhould be *bouched* or lined with gold, in the ſame manner as the touch-hole is made in the better kind of fowling-pieces, in order that its dimenſions may not be increaſed by repeated firing.

The bullets ſhould be made to fit the bore with very little windage; and it would be better if they were all caſt in the ſame mould, and of the ſame parcel of lead, as in that caſe their weights and dimenſions would be more accurately the ſame, and the experiments would of courſe be more concluſive.

The ſtated charge of powder may be half an ounce, and it ſhould always be put up in a cartridge of very fine paper; and after the piece is loaded it ſhould be primed with other powder, firſt taking care to prick the cartridge by thruſting a priming wire down the vent.

As it appears, from ſeveral experiments made on purpoſe to aſcertain the fact, that ramming the powder more or leſs has a very ſenſible effect to increaſe or diminiſh the force of the charge; to prevent any inaccuracies that might ariſe from that cauſe, a ram-rod, ſuch as is repreſented fig. 15. may be made uſe of. It is to be made of a cylindrical piece of wood in the ſame manner as ram-rods in general are made, but with the addition of a ring, C, about one inch and a half, or two inches in diameter, which being placed at a proper diſtance from the end (*a*) of the ram-rod

that goes up into the bore, will prevent its being thruſt up too far. This ring may be made of wood, or of any kind of metal, as ſhall be found moſt convenient. The other end of the ram-rod (*b*) may be 31 or 32 inches in length from the ring, and the extremity of it being covered with a proper ſubſtance, it may be made uſe of for wiping out the barrel after each experiment.

The machine (*f*) for the tape to ſlide through may be the ſame as that deſcribed by Dr. HUTTON in his account of his experiments on the initial velocities of cannon balls; as his method is much better calculated to anſwer the purpoſe than that propoſed and made uſe of by Mr. ROBINS. It will alſo be better for the axis of each of the pendulous rods to reſt upon level pieces of wood or iron, than for them to move in circular grooves: care muſt however be taken to confine them by ſtaples, or by ſome other contrivance, to prevent their ſlipping out of their places.

The trunnions, by means of which the barrel is connected with the pendulous rods, and upon which it is ſupported, ſhould be as ſmall as poſſible, in order to leſſen the friction; and for the ſame reaſon they ſhould be well poliſhed, as well as the grooves which receive them. They need not be caſt upon the barrel, but may be ſcrewed into it after it is finiſhed.

In making the experiments, regard muſt be had to the heat of the barrel, as well as to the temperature and ſtate of the atmoſphere; for heat and cold,—dryneſs and moiſture,—have very ſenſible

effects

effects upon gun-powder, to increaſe or diminiſh its force. If therefore, a very great degree of accuracy is at any time required, it will be beſt to begin by firing the piece two or three times merely to warm it; after which, three or four experiments may be made with ſtandard powder, to determine anew the proof mark (for the ſtrength of the ſame powder is different upon different days); and when this is done, the experiments with the powder that is to be proved are to be made, taking care to preſerve the ſame interval of time between the firings, that the heat of the piece may be the ſame in each trial.

If all theſe precautions are taken, and if the bullets are of the ſame weight and dimenſions, powder may be proved by this method with much greater accuracy than has hitherto been done by any of the methods hitherto uſed for that purpoſe.

Of the comparative goodneſs, or value, of powder of different degrees of ſtrength.

Let V denote the velocity of the bullet with the ſtronger powder, and put v equal to the velocity with the weaker, when the charges are equal, and the weight and dimenſions of the bullets are the ſame, and when they are diſcharged from the ſame piece. If the charge is augmented when the weaker powder is made uſe of, till the velocity of the bullet is increaſed from v to V, or till it becomes equal to the velocity with the given charge of the ſtronger powder, the *value* of the charges may then be ſaid to be equal; and conſequently the

weaker powder is as much worſe than the ſtronger, —or is of leſs value—in proportion as the quantity of it, by the pound, required to produce the given effect, is greater.

But we have ſeen, that the velocities, with different quantities of the ſame kind of powder, are in the *ſub-duplicate ratio* of the weights of the charges. The charges, therefore, muſt be as the ſquares of the velocities, and conſequently the charge of the weaker powder muſt be to that of the ſtronger, when the velocities are equal, as VV is to *vv*. The weaker powder is therefore as much worſe than the ſtronger, as VV is greater than *vv*; or the comparative goodneſs of powder, of different degrees of ſtrength, is as the ſquares of the velocities of the bullets, when the charges are equal.

The mean velocity of the bullets, as ſhewn by the pendulum, in the 104th and 105th experiments, when the piece was fired with 145 grains of government powder, was 894 feet in a ſecond; and with the ſame quantity of *double proof** battle powder (experiment N° 106) the velocity was 990 feet in a ſecond. Now the ſquares of theſe velocities, (which, as we juſt obſerved, meaſure the goodneſs of the powder) are to each other as 1 is to 1,2263, or nearly as 5 is to 6.

With 218 grains of government powder, the mean velocity in four experiments (*viz.* the 40th, 41ſt, 42d, and 43d) was 1225 feet in a ſecond;

* This is called *battle* powder, not becauſe it is uſed in battle or in war; but from *Battle*, the name of a village in Kent, where that kind of powder is made.

and

and in the experiment N° 107, when the ſame quantity of *double proof* battle powder was made uſe of, the velocity was 1380 feet in a ſecond; and $\overline{1225}^2$ is to $\overline{1380}^2$ as 1 is to 1,2691.

With 290 grains, or half the weight of the bullet in government powder in the 109th, 110th, 111th, and 112th experiments, the mean velocity of the bullet was 1444 feet in a ſecond; but with the ſame quantity of the battle powder (experiment N° 116) the velocity was 1525 feet in a ſecond; $\overline{1444}^2$ is to $\overline{1525}^2$, as 1 is to 1,1153.

By taking a medium of theſe trials it appears, that double proof battle powder is better than government powder in proportion as 1,2036 is to 1, or nearly as 6 is to 5.

But if, inſtead of weighing the powder, we eſtimate the quantity of the charge by meaſurement, or the ſpace it occupies in the bore of the piece, the comparative ſtrength of battle powder will appear to be conſiderably greater, or its ſtrength will be to that of government powder nearly as 4 is to 3; for the grains of this better kind of powder being more compact and nearly of a ſpherical form, a greater weight of it will lie in any given ſpace, than of government powder, which is formed more looſely, and of various, and of very irregular figures.

Now the common price of double proof battle powder, as it is ſold by the wholeſale dealers in that commodity, is at the rate of £. 10 *per* cwt. net, which is juſt two ſhillings by the pound; while government is ſold at £. 5 5*s*. *per* hundred, or one

ſhilling and $\frac{6}{10}$th of a penny *per* pound; but battle powder is better than government powder only in the proportion of 1,2036 to 1, or of one ſhilling and two pence, to one ſhilling and $\frac{6}{10}$th of a penny; battle powder is therefore ſold at the rate of ten pence by the pound, or $41\frac{2}{3}$ *per cent.* dearer than it ought to be; or thoſe, who make uſe of it, in preference to government powder, do it at a certain loſs of $41\frac{2}{3}$ *per cent.* of the money that the powder coſts them.

Of the relation of the velocities of bullets to their weights.

According to Mr. ROBINS's theory, when bullets of the ſame diameter, but different weights, are diſcharged from the ſame piece, by the ſame quantity of powder, their *velocities* ſhould be in the *reciprocal ſub-duplicate ratio of their weights*; but as this theory is founded upon a ſuppoſition that the action of the elaſtic fluid, generated from the powder, is always the ſame, in any, and every given part of the bore, when the charge is the ſame, whatever may be the weight of the bullet; and as no allowance is made for the expenditure of force required to put the fluid itſelf in motion, or for the loſs of it by the vent; it is plain that the theory is defective. It is true, Dr. HUTTON, in his experiments, found this law to obtain, without any great error, and poſſibly it may hold good, with ſufficient accuracy, in many caſes; for it ſometimes happens, that a number of errors, or actions, whoſe operations have

have contrary tendencies, ſo compenſate each other, that their effects, when united, are not ſenſible. But when this is the caſe, if any one of the cauſes of error be removed, thoſe which remain will be detected.

When any given charge is loaded with a heavy bullet, more of the powder is inflamed in any very ſhort ſpace of time than when the bullet is lighter, and the action of the powder ought, of courſe, to be greater on that account; but then, a heavy bullet takes up more time in paſſing through the bore than a light one, and conſequently more of the elaſtic fluid, generated from the powder, eſcapes by the vent and by windage. It may happen, that the augmentation of the force, on account of one of theſe circumſtances, may exactly counterbalance the diminution of it ariſing from the other; and if it ſhould be found, upon trial, that this is the caſe in general, in pieces as they are now conſtructed, and with all the variety of ſhot that are made uſe of in practice, it would be of great uſe to know the fact: and poſſibly it might anſwer as well, as far as it relates to the art of gunnery, as if we were perfectly acquainted with, and were able to appreciate, the effect of each varying circumſtance under which an experiment can be made. But when, concluding too haſtily from the reſult of a partial experiment, we ſuppoſe with Mr. Robins, that, becauſe the ſum total of the action, or preſſure, of the elaſtic fluid upon the bullet, during the time of its paſſage through the bore, happens to be the ſame when bullets of different weights are made uſe of

(which

(which collective preſſure is in all caſes proportional to, and is accurately meaſured by, the velocity, or rather motion, communicated to the bullet) that *therefore*, the preſſure in any given part is always exactly the ſame, when the quantity of powder is the ſame with which the piece is fired; and from thence endeavour to prove, that the inflammation of gun-powder is inſtantaneous, or that the whole charge is, in all caſes, inflamed, and "converted *in-*" "to an elaſtic fluid before the bullet is ſenſibly "moved from its place;" ſuch reaſonings and concluſions may lead to very dangerous errors.

It is undoubtedly true, that if the principles aſſumed by Mr. ROBINS with reſpect to the manner in which gun-powder takes fire, and the relation of the elaſticity of the generated fluid to its denſity, or the intenſity of its preſſure upon the bullet as it expands in the barrel, were juſt, and if the loſs of force by the vent, and by windage, were in all caſes inconſiderable, or if it were prevented, the deductions from the theory reſpecting the velocities of bullets of different weights would always hold good. But if, on the contrary, it ſhould be found, upon making the experiments carefully, and in ſuch a manner as entirely to prevent inaccuracies ariſing from adventitious circumſtances, that the velocities obſerve a law different from that which the theory ſuppoſes, we may fairly concludè, that the principles upon which the theory is founded are erroneous.

Let us now ſee how far theſe experiments differ from the theory. Thoſe numbered from 84 to 92 incluſive were made in ſuch a manner that no part

of

of the force of the powder was loſt by the vent, or by windage, as has already been mentioned, and all poſſible attention was paid to every circumſtance that could contribute to render them perfect and concluſive.

A particular account has already been given of them, and notice has been taken of the means that were uſed for forming the bullets, and making them fit the bore, and of the contrivance for preventing the eſcape of the elaſtic fluid by the vent. The following table ſhews the reſults of them.

N. B. The charge of powder was the ſame in each experiment, and conſiſted of 145 grains in weight.

	Weight of the bullet.	Velocity of the bullet.		
		Actual.	Computed.	Difference.
85th exp.	90 grs.	2109	2109	———
86th	251	1430	1262	+ 168
87th	354	1288	1063	+ 225
88th	600	1240	817	+ 423
89th	603	1224	815	+ 409
90th	1184	1017	581	+ 436
91ſt	1754	893	478	+ 415
92d	2352	812	413	+ 399

The computed velocities, as they are ſet down in this table, were determined from the actual velocity of the bullet, as determined by the recoil, in the 85th experiment, and the reciprocal ſub-duplicate ratio of its weight to the weight of the bullet in each ſubſequent experiment; and in the laſt column

is

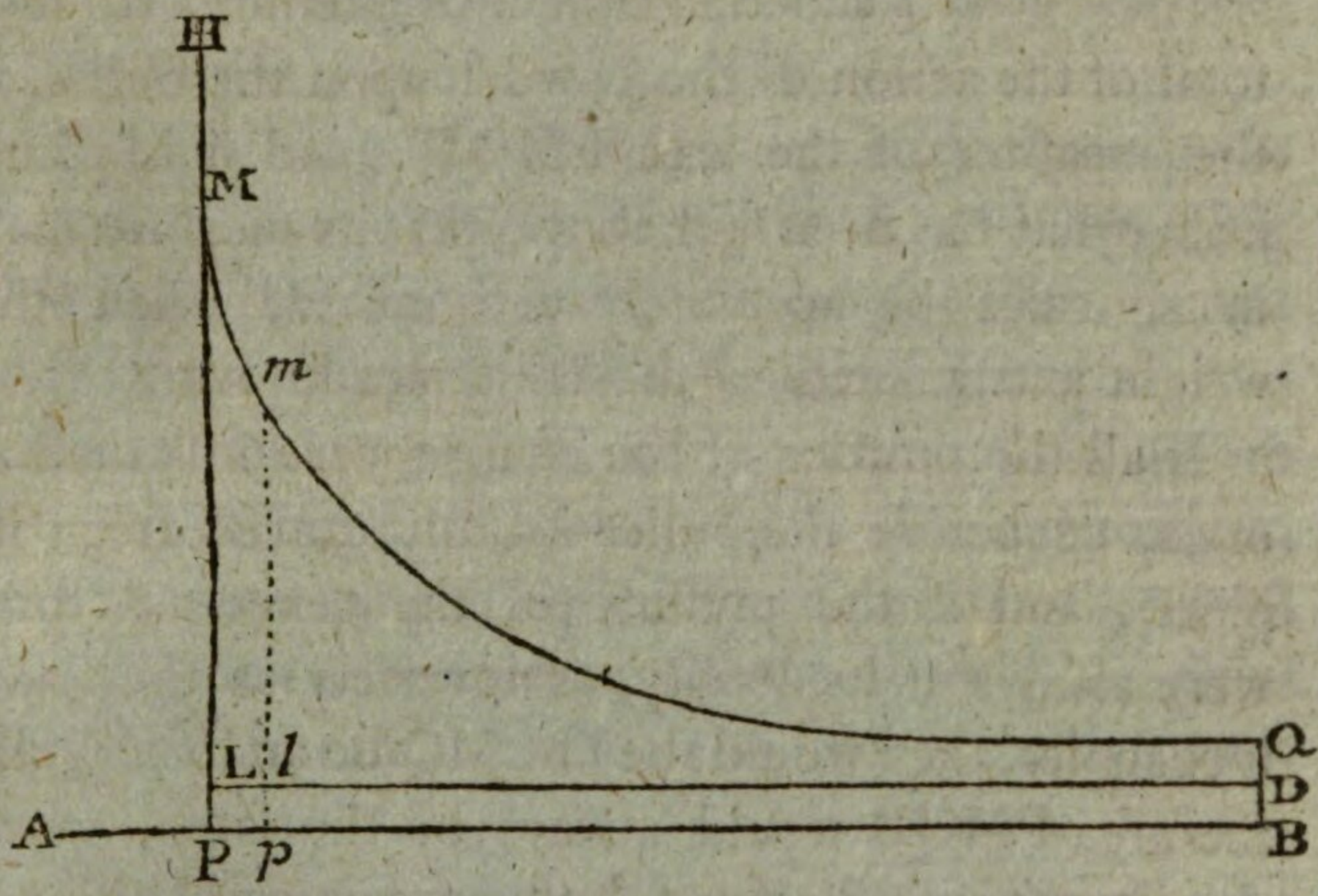
is marked the difference between the experiment and the theory, or the number of feet in a ſecond, by which the actual velocity exceeded the computed.

But in order that we may ſee this matter in different points of view, let the order of the experiments be now inverted, and let the computed velocities be determined from the actual velocity in the 92d experiment; and aſſuming the total, or collective preſſure exerted by the powder upon the bullet, in that experiment, equal to unity, let the collective preſſure in the other experiments be computed from the ratio of the actual to the computed velocities, and the table will ſtand thus:

	Weight of the bullet.	Velocity of the bullet. Actual.	Comp.	Difference	Collective preſſure.
92d exp.	2352 grs.	812	812	———	1,0000
91ſt	1754	893	940	— 47	0,9020
90th	1184	1017	1145	— 128	0,7897
89th	603	1224	1604	— 380	0,5825
88th	600	1240	1608	— 368	0,5949
87th	354	1288	2093	— 805	0,3778
86th	251	1430	2486	— 1056	0,3310
85th	90	2109	4151	— 2042	0,2581

In

In the following figure let AB repreſent the axis of the piece, and AP the length of the ſpace filled with powder; and at the point P let the perpendicular



PH be erected, upon which let PL and PM be taken from P towards H of ſuch magnitudes that while PL expounds the uniform force of gravity, or the weight of the bullet, PM ſhall be as the force exerted by the powder upon the bullet at the moment of the exploſion. If now we ſuppoſe, that while the bullet moves on from P towards B, the line PM or *pm*, goes along with it, and that the point *m* is always taken in ſuch a manner that the line *pm*, ſhall be to *pl*, or PL, as the force acting upon the bullet in the point *p*, is to its weight, till *pm*, coincides with QB, then will the area PMQB be to the area PLDB in the duplicate proportion of the velocities which the bullet would acquire when acted on by its own gravity through the ſpace PB, and when impelled through the ſame ſpace by the force

force of the powder, as may be ſeen demonſtrated by Sir ISAAC NEWTON, in his Mathematical Principles of Natural Philoſophy, book I. prop. 39.

Now what I call the collective preſſure, or ſum total of the action of the powder upon the bullet, is the meaſure of the area PMQB; and it is plain, from what has been ſaid above, that its meaſure may in all caſes be accurately determined, when the weight and velocity of the bullet are known.

If all the powder of the charge were inflamed at once, or before the bullet ſenſibly moved from its place; and if the preſſure of the generated fluid were always as its denſity, or inverſely as the ſpace it occupies, then would the line MQ be an hyperbola, the area PMQB would always be the ſame, when the charge was the ſame, and conſequently the velocities of the bullets would be as the ſquare roots of their weights, inverſely. But it appears, from the before mentioned experiments, that when the weight of the bullet was increaſed four times, the action of the powder, or area PMQB, was nearly doubled; for in the 92d experiment, when four bullets were diſcharged at once, the collective preſſure was as 1; but in the 89th experiment, when a ſingle bullet was made uſe of, the collective preſſure was only as 0,5825; and in the 85th, 86th, and 87th experiments, when the bullets were much lighter, the action of the charge on them was ſtill leſs.

But though we can determine, with great certainty, from theſe experiments, *the ratio in which the action of the powder upon the bullet was increaſed or diminiſhed*, by making uſe of bullets of greater

or

or leſs weight; yet we cannot from thence aſcertain the relation of the elaſticity of the generated fluid, to its denſity, nor the quantity of powder that is inflamed at different periods before and after the bullet begins to move in the bore.

But aſſuming Mr. ROBINS's principles, as far as relates to the elaſticity of the fluid, and ſuppoſing that in all the experiments, except the 92d, a part only of the charge took fire, and that that part was inflamed and converted into an elaſtic fluid before the bullet began to move; upon that ſuppoſition, we can determine the quantity of powder that took fire in each experiment; for the quantity of powder, in that caſe, would be as the collective preſſure.

Thus, if the whole charge, = 145 grains in weight, is ſuppoſed to have been inflamed in the 92d experiment, the quantity inflamed in each of the other experiments will appear to have been as follows, *viz.*

	Weight of the bullet.	Velocity of the bullet.	Collective preſſure.	Powder inflamed.
85th exp.	90 grs.	2109	0,2581	37 grs.
86th	251	1430	0,3310	48
87th	354	1288	0,3778	55
88th	600	1240	0,5949	86
89th	603	1224	0,5825	84
90th	1184	1017	0,7897	114
91ſt	1754	893	0,9020	131

But

But there are many reaſons to ſuppoſe, that the diminution of the action of the powder upon the bullet, when it is lighter, is not ſo much owing to the ſmallneſs of the quantity of powder that takes fire in that caſe, as to the *vis inertiæ* of the generated fluid. It is true, that a greater portion of the charge takes fire and burns when the bullet is heavy than when it is light, as I found in the very experiments of which I am now ſpeaking; but then, the quantity of unfired powder, in any caſe, was much too ſmall to account for the apparent diminution of the force, when light bullets were made uſe of.

If the elaſtic fluid, in the action of which the force of powder conſiſts, were infinitely rare, or if its weight bore no proportion to that of the powder that generated it; and if the groſs matter, or *caput mortuum* of the powder, remained in the bottom of the bore, after the exploſion, then, and upon no other ſuppoſition, would the preſſure upon the bullet be inverſely as the ſpace occupied by the fluid: but it is evident that this can never be the caſe.

A curious ſubject for ſpeculation here occurs: how far would it be advantageous, were it poſſible, to diminiſh the ſpecific gravity of gun-powder, and the fluid generated from it, without leſſening its elaſtic force? It would certainly act upon very light bullets with greater force; but when heavy ones came to be made uſe of, there is reaſon to think, that, except extraordinary precaution was taken to prevent it, the greateſt part of the force would be loſt by the vent, and by windage.

The

The velocity with which elaſtic fluids ruſh into a void ſpace, is as the elaſticity of the fluid directly, and inverſely as its denſity; if, therefore, the denſity of the fluid, generated from powder, were four times leſs than it is, its elaſticity remaining the ſame, it would iſſue out at the vent, and eſcape by the ſide of the bullet, in the bore, with nearly four times as great a velocity as it does at preſent; but we know from experiment that the loſs of force on thoſe accounts is now very conſiderable.

In the experiments N°s 76 and 77, when the piece was fired with 145 grains of powder, the velocity of the bullets, at a medium, was 1040 feet in a ſecond; but in the 88th and 89th experiments, when the bullets were even heavier, and the piece was fired with the ſame quantity of powder, the mean velocity was 1232 feet in a ſecond. The difference = 192 feet in a ſecond, anſwers to a difference of force greater in the laſt experiments than in the firſt in the proportion of .14 to 10.

I know of no way to account for this difference, but by ſuppoſing that it was owing entirely to the eſcape of the elaſtic fluid by the vent, and by windage, in thoſe experiments where the vent was open, and the bullets were put naked into the piece.

An elaſtic bow, made of very light wood, will throw an arrow, and eſpecially a light one, with greater velocity than a bow of ſteel of the ſame degree of ſtiffneſs: but, for practice, I think it is plain, that gun-powder may *be ſuppoſed* to be ſo light as to be rendered entirely uſeleſs: and for

ſome purpoſes it ſeems probable, that it would not be the worſe for being even heavier than it is now made. Vents are abſolutely neceſſary in fire-arms, and in large pieces of ordnance, the windage muſt be conſiderable, in order that the bullets, which are not always ſo round as they ſhould be, may not ſtick in the bore; and thoſe who have been preſent at the firing of heavy artillery, and large mortars, with ſhot and ſhells, muſt have obſerved, that there is a ſenſible ſpace of time elapſes between the lighting of the prime, and the exploſion; and that, during that interval, the flame is continually iſſuing out at the vent with a hiſſing noiſe, and with a prodigious velocity, as appears by the height to which the ſtream of fire mounts up in the air. It is plain, that this loſs muſt be greater in proportion as the ſhot that is diſcharged is heavier; and I have often fancied, that I perceived a ſenſible difference in the time that elapſed between the firing of the prime, and the exploſion, when bullets were diſcharged, and when the piece has been fired with powder only; the time being apparently longer in the former caſe than the latter.

Almoſt all the writers upon gun-powder, and particularly thoſe of the laſt century, gave different *recipes* for powder that is deſigned for different uſes. Thus the French authors mention, *poudre a mouſquet*, *poudre ordinaire de guerre*, *poudre de chaſſe*, and *poudre d'artifice*; all of which are compoſed of nitre, ſulphur, and charcoal, taken in different proportions. Is it not probable, that this variety in the compoſition of powder was originally introduced

introduced in conſequence of obſervation that one kind of powder was better adapted for particular purpoſes than another, or from experiments made on purpoſe to aſcertain the fact? There is one circumſtance that would lead us to ſuppoſe that that was the caſe.—That kind of powder which was deſigned for great guns and mortars was weaker than thoſe which were intended to be uſed in ſmaller pieces; for if there is any foundation for theſe conjectures, it is certain, that the weakeſt powder, or the heavieſt in proportion to its elaſtic force, ought to be uſed to impel the heavieſt bullets, and particularly in guns that are imperfectly formed, where the vent is large, and the windage very great.

I am perfectly aware, that an objection may here be made, viz. that the elaſtic fluid, which is generated from gun-powder, muſt be ſuppoſed to have the ſame properties, very nearly, whatever may be the proportion of the ſeveral ingredients, and that therefore, the only difference there can be in powder is, that one kind may generate more of this fluid, and another leſs; and that when it is generated, it acts in the ſame manner, and will alike eſcape, and with the ſame velocity, by any paſſage it can find. But to this I anſwer, though the fluid may be the ſame, as undoubtedly it is, and though its denſity and elaſticity may be the ſame in all caſes, at the inſtant of its generation, yet, in the exploſion, the elaſtic and un-elaſtic parts are ſo mixed and blended, that I imagine the fluid cannot expand without taking the groſs matter along with it, and the velocity with which the flame iſſues out

at the vent is to be computed from the elaſticity of the fluid, and the denſity or weight of the fluid and the groſs matter taken together, and not ſimply from the elaſticity and denſity of the fluid. If antimony, in an impalpable powder, or any other heavy body, were intimately mixed with water, in a veſſel of any kind, and kept in ſuſpenſion by ſhaking or ſtirring them about; and if a hole were opened in the ſide or bottom of the veſſel, the water would not run out without taking the particles of the ſolid body along with it. And in the ſame manner I conceive the ſolid particles that remain after the exploſion of gun-powder to be carried forward with the generated elaſtic fluid, and being carried forward retard its motion.—But to return from this digreſſion.

As it appears from theſe experiments, that the relation of the velocities of bullets to their weights is different from that which Mr. Robins's theory ſuppoſes, it remains to inquire *what the law is which actually obtains*. And, firſt, as the velocities bear a greater proportion to each other than the reciprocal ſubduplicate ratio of the weights of the bullets, let us ſee how near they come to the reciprocal ſub-triplicate ratio of their weights.

Weight of the bullet.	Velocity of the bullet. Computed. Recip. ſub-dup. ratio.	Actual. Error of the theory.	Computed. Error of the theory.	Computed. Recip. ſub-trip. ratio.
92d	2352	812 ——	812 ——	812
91ſt	1754	940 + 47	893 + 2	895
90th	1184	1145 + 128	1017 + 4	1021
89th	603	1604 + 380	1224 + 54	1278
88th	600	1608 + 368	1240 + 40	1280
87th	354	2093 + 805	1288 + 239	1527
86th	251	2486 + 1056	1430 + 282	1712
85th	90	4151 + 2042	2109 + 301	2410

Here the velocities computed upon the laſt ſuppoſition appear to agree much better with the experiments than thoſe computed upon Mr. ROBINS's principles; but ſtill there is a conſiderable difference between the actual and the computed velocities in the three laſt experiments in the table.

As the powder itſelf is heavy, it may be conſidered as a weight that is put in motion along with the bullet; and if we ſuppoſe the denſity of the generated fluid is always uniform from the bullet to the breech, the velocity of the center of gravity of the powder or (which amounts to the ſame thing) of the elaſtic fluid, and the groſs matter generated from it will be juſt half as great as the velocity of the bullet. If therefore we put P to denote the weight of the powder, B the weight of the bullet, and v its initial velocity; then $B\,v + \frac{1}{2}\,P\,v = \overline{B + \frac{1}{2}\,P} \times v$ will expreſs the *momentum* of the charge at the inſtant when the bullet quits the bore.

If now, inſtead of aſcertaining the relation of the velocities to the weights of the bullets, we add half

the weight of the powder to the weight of the bullet, and compute the velocities from the reciprocal ſub-triplicate ratio of the quantity $\overline{B + \frac{1}{2}P}$ in each experiment, the table will ſtand thus:

	Weight of the bullet and half the powder. $\overline{B + \frac{1}{2}P} =$	Velocity of the bullet. Actual,	Computed.	Error of the theory.
92d exp.	$2352 + 72\frac{1}{2}$	812	812	——
91ſt	$1754 + 72\frac{1}{2}$	893	892	— 1
90th	$1184 + 72\frac{1}{2}$	1017	1011	— 6
89th	$603 + 72\frac{1}{2}$	1224	1243	+ 19
88th	$600 + 72\frac{1}{2}$	1240	1245	+ 5
87th	$354 + 72\frac{1}{2}$	1288	1449	+ 161
86th	$251 + 72\frac{1}{2}$	1430	1589	+ 159
85th	$90 + 72\frac{1}{2}$	2109	1999	— 110

The agreement between the actual and computed velocities is here very remarkable, and particularly in the five firſt experiments, which are certainly thoſe upon which the greateſt dependence may be placed.

And hence we are enabled to determine the natures of the curves, *mn*, and *gf* (fig. 16.); for ſince B (which expreſſes the weight of the bullet) is as the length taken from A towards B, in the ſeveral experiments; and as the velocities are as the lines drawn perpendicular to the line AB from the places where thoſe lengths terminate, as *w*, *u*, &c. ending at the curve, *m*, *n*; if we put $a = \frac{1}{2}P$, $x = B$,

$x = B$, and $y = wu$; then will the relation of x and y be defined by this equation $\frac{1}{\sqrt{a+x^3}} = y$. And if z be put to denote the line $w\,r$, and b, the recoil when the given charge is fired without any bullet, it will be $\frac{x}{\sqrt{a+x^3}} + b = z$ in the curve $g\,f$, x being the abſciſſa, and z the correſponding ordinate to the curve.

In the 92d experiment half the weight of the powder ($=a$) was $72\frac{1}{2}$ grains; the weight of the bullet was 2352 grains ($=x$;) the recoil ($=z$) was 32,25 inches, and with the given charge without any bullet the recoil ($=b$) was 4,4 inches; if now from theſe *data*, and the known weight of the bullet in each of the other experiments in this ſet, the recoil be computed by means of the theorem $\frac{x}{\sqrt{a+x^3}} + b = z$, we ſhall ſee how the reſult of thoſe experiments agrees with this theory: thus,

	Weight of the bullet.	Recoil: Actual.	Recoil: Computed.	Difference.
92d exp.	2352	32,25	32,25	
91ſt	1754	27,18	27,22	+0,04
90th	1184	21,92	21,85	−0,07
89th	603	15,13	15,33	+0,2
88th	600	15,22	15,29	+0,07
87th	354	11,03	11,87	+0,84
86th	251	9,62	10,21	+0,59
85th	90	7,16	7,02	−0,14
84th & 93d	0	4,4	4,4	

Here the agreement of the actual and computed recoils is as remarkable as that of the actual and computed velocities in the foregoing table.

By the figure 17. may be seen at one view the result of all these experiments and computations. The numbers upon the line AB (as in the fig. 16.) represent the weights of the bullets, while the lines drawn from those numbers perpendicular to AB on each side, and ending at the curves *m*, *n*, are as the velocities of the bullets in the several experiments; the line AB being the axis of the curves, the lengths taken from A to the different numbers towards B (=*x*) the abscissa, and the perpendiculars (=*y*) the corresponding ordinates. The ordinates to the curve *hn*, are as the velocities computed from the theorem $\frac{1}{\sqrt{a+x^3}}=y$, and the ordinates to the curve *p*, *n* (which is the logarithmic curve, as it is $\frac{1}{\sqrt{x}}=y$) shew the velocities computed upon Mr. ROBINS's principles. The curve *gf* is drawn from the theorem $\frac{x}{\sqrt{a+x^3}}+b=z$; and the actual recoil is marked upon the ordinates to this curve by large round dots, which in all the experiments, except the 86th and 87th, very nearly coincide with the curve.

In the fig. 18. the numbers upon the line AB, taken from A, denote the different charges of powder used in the course of the experiments, while the ordinates to the curve *cd*, express the velocities of the bullets, with the vent at o. The lines drawn perpendicular from the line AB to the line *ef*, represent the recoil with the several charges of powder,

der, and a leaden bullet; and the portion of thoſe lines that is comprehended between the line AB and the line *gh*, denotes the recoil when the given charge was fired without any bullet.

Having now ſhewn by experiment the relation of the velocities of bullets to their weights, when care is taken to prevent entirely the loſs of force by the eſcape of the elaſtic fluid through the vent, and by the windage, I ſhall leave it to mathematicians to determine, from theſe *data*, the properties of that fluid, or the relation of its elaſticity to its denſity.

But, before I take my leave of this ſubject, I would juſt obſerve, that Mr. ROBINS is not only miſtaken in the principle he aſſumes, reſpecting the relation of the elaſticity of the fluid generated from gun-powder to its denſity, or rather the law of its action upon the bullet, as it expands in the bore; but his determination of the force of gun-powder is alſo erroneous, even upon his own principles: for he determines its force to be 1000 times greater than the mean preſſure of the atmoſphere; whereas it appears, from the reſult of the 92d experiment, that its force is at leaſt 1308 times greater than the mean preſſure of the atmoſphere, as will be evident to thoſe who will take the trouble to make the computation.

Of an attempt to determine the exploſive force of aurum fulminans, *or a compariſon between its force, and that of gun-powder.*

Having provided myſelf with a ſmall quantity of this wonderful powder, upon the goodneſs of which I could

I could depend, I endeavoured to aſcertain its exploſive force, by making uſe of it inſtead of gunpowder, for diſcharging a bullet, and meaſuring, by means of the pendulum, the velocity which the bullet acquired; and concluding, from the tremendous report with which this ſubſtance explodes, that its elaſtic force was vaſtly greater than that of gun-powder, I took care to have a barrel provided of uncommon ſtrength, on purpoſe for the experiment. Its length, in the bore, is 13,25 inches, the diameter of the bore is 0,55 of an inch, and its weight 7 lbs. 2 oz. It is of the beſt iron, and was made by WOGDON; and the accuracy with which it is finiſhed does credit to the workman.

This barrel being charged with one ſixteenth of an ounce (= 27,34 grains) of *aurum fulminans*, and two leaden bullets, which, together with the leather that was put about them to make them fit the bore without windage, weighed 427 grains; it was laid upon a chaffing-diſh of live coals, at the diſtance of about 10 feet from the pendulum, and againſt the center of the target of the pendulum the piece was directed.

Having ſecured the barrel in ſuch a manner that its direction ſhould not alter, I retired to a little diſtance, in order to be out of danger in caſe of an accident, where I waited in anxious expectation, the event of the exploſion.

I had remained in this ſituation ſome minutes, and almoſt deſpaired of the experiment's ſucceeding, when the powder exploded, but with a report infinitely leſs than what I expected; the noiſe not greatly

greatly exceeding the report of a well-charged wind-gun; and it was not till I saw the pendulum in motion that I could be perſuaded that the bullets had been diſcharged. I found, however, upon examination, that nothing was left in the barrel, and from the great number of ſmall particles of revived metal that were diſperſed about, I had reaſon to think that all the powder had exploded.

The bullets ſtruck the pendulum nearly in the center of the target, and both of them remained in the wood; and I found, upon making the calculation, that they had impinged againſt it with a velocity of 428 feet in a ſecond.

If we now ſuppoſe that the force of *aurum fulminans* ariſes from the action of an elaſtic fluid that is generated from it in the moment of its exploſion, and that the elaſticity of this fluid, or rather the force it exerts upon the bullet as it goes on to expand, is always as its denſity, or inverſely as the ſpace it occupies; then, from the known dimenſions of the barrel,—the length of the ſpace occupied by the charge (which in this experiment was 0,47 of an inch),—and the weight and velocity of the bullets, the elaſtic force of this fluid, at the inſtant of its generation, may be determined: and I find, upon making the calculation upon theſe principles, that its force turns out 307 times greater than the mean elaſtic force of common air.

According to Mr. Robins's theory, the elaſtic force of the fluid generated from gun-powder in its combuſtion, is 1,000 times greater than the

mean

mean preſſure of the atmoſphere; the force of *aurum fulminans*, therefore, appears to be to that of gun-powder as 307 is to 1,000, or as 4 is to 13 very nearly.

Of the ſpecific gravity of gun-powder.

To determine the ſpecific gravity of gun-powder, I made uſe of the following method. A large glaſs bucket, with a narrow mouth, being ſuſpended to one of the arms of a very nice balance, and counter-poiſed by weights put in the oppoſite ſcale, it was filled firſt with government powder poured in lightly,—then with the ſame powder ſhaken well together,—afterwards with powder and water together, and laſtly with water alone, and in each caſe the contents of the bucket were very exactly weighed.

The ſpecific gravity of gun-powder, as determined from theſe experiments, is as follows:

Specific gravity of rain water - -	1,000
Government powder, as it lies in a heap mixed with air - - - - -	0,836
Government powder well ſhaken together -	0,937
The ſolid ſubſtance of the powder - -	1,745

Hence it appears, that a cubic inch of government powder, ſhaken well together, weighs juſt 243 grains; that a cubic inch of ſolid powder would weigh 422 grains; and, conſequently, that the interſtices between the particles of the powder, as it is grained for uſe, are nearly as great as the ſpaces which thoſe particles occupy.

MISCEL-

MISCELLANEOUS EXPERIMENTS.

Of ſome unſucceſsful attempts to increaſe the force of gun-powder.

It has been ſuppoſed by many, that the force of ſteam is even greater than that of gun-powder; and that if a quantity of water, confined in the chamber of a gun, could at once be rarified into ſteam, it would impel a bullet with prodigious velocity. Several attempts have been made to ſhoot bullets in this manner; but I know of none that have ſucceeded; at leaſt ſo far as to render it probable that water can ever be ſubſtituted in the room of gun-powder, for military purpoſes, as ſome have imagined.

The great difficulty that attends making theſe experiments lies in finding out a method by which the water can at once be rarified and converted into elaſtic ſteam; and it occurred to me, that poſſibly that might be effected by means of gun-powder, by confining a ſmall quantity of water in ſome very thin ſubſtance, and ſurrounding and incloſing it with powder, and afterwards ſetting fire to the charge. The method I took to do this was, as follows. Having procured a number of air-bladders of very ſmall fiſhes, I put different quantities of water into them, from the ſize of a ſmall pea, to that of a ſmall piſtol bullet, and tying them up cloſe, with ſome very fine thread, I hung up theſe little globules in the open air, till they

were

were quite dry on the outside. I then provided a number of cartridges, made of fine paper, and filled them with a known quantity of powder, equal to the customary charge for a common horseman's pistol; and having loaded such a pistol with one of them and a fit bullet, I laid it down upon the ground, and directing it against an oaken plank that was placed about six feet from the muzzle, I fired it off by a train, and carefully observed the recoil, and also the penetration of the bullet. I then took several of the filled cartridges that remained, and pouring out part of the powder, I put one or more of the little bladders filled with water in the centre of the cartridge, and afterwards pouring back the remaining part of the charge, confined the water in the midst of the powder.

With these cartridges and a fit bullet, the pistol was successively loaded, and being placed on the ground as before, and fired by a train, the recoil, and the penetration of the bullets were observed; and I constantly found, that the force of the charge was very sensibly diminished by the addition of the globule of water, and the larger the quantity of water was, that was thus confined, the less was the effect of the charge; neither the recoil of the pistol, nor the penetration of the bullet, being near equal to what they were when the given quantity of powder was fired without the water; and the report of the explosion appeared to be lessened in a still greater proportion than the recoil or penetration.

Concluding that this diminution of the force of the

the charge arose from the bursting of the little bladder, and the dispersion of the water among the powder before it was all inflamed, by which a great part of it was prevented from taking fire, I repeated the experiments with highly rectified spirits of wine instead of water; but the result was nearly the same as before; the force of the charge was constantly, and very sensibly, diminished. I afterwards made use of etherial oil of turpentine, and then of small quantities of quicksilver; but still with no better success. Every thing I mixed with the powder, instead of increasing, served to lessen the force of the charge.

These trials were all made several months before I began the course of my experiments upon gunpowder, of which I have here given an account, and though they were altogether unsuccessful, yet I resumed the inquiry at that time, and made several new experiments, with a view to find out something that should be stronger than gun-powder, or which, when mixed with it, should increase its force.

It is well known, that the elastic force of quicksilver, converted into vapour, is very great; this substance I made use of in my former trials, as I have just observed, but without success. I thought, however, that the failure of that attempt, might possibly be owing to the quicksilver being too much in a body, by which means the fire could not act upon it to the greatest advantage: but that if it could be divided into exceeding small particles, and so ordered that each particle might

be completely furrounded by, and expofed to, the action of the flame of the powder, it would be very ſoon heated, and poſſibly might be converted into an elaſtic ſteam or vapour, before the bullet could be ſenſibly removed from its place. To determine this point I mixed 20 grains of æthiops mineral very intimately with 145 grains of powder, and charging the piece with this compound, it was loaded with a fit bullet and fired; but the force of the charge was leſs than that which the powder alone would have exerted, as appears by comparing the 76th and 77th experiments with the 79th.

Common *pulvis fulminans* is made of one part of ſulphur, two parts of ſalt of tartar, and three parts of nitre; and if we may judge by the report of the exploſion, the elaſtic force of this compound is conſiderably greater than that of gun-powder. I was willing to ſee the effect of mixing ſalt of tartar with gun-powder, and accordingly, having provided ſome of this alkaline ſalt, in its pureſt ſtate, thoroughly dry, and in a fine powder, I mixed 20 grains of it with 145 grains of gun-powder; and upon diſcharging the bullet with the mixture, I found that the alkaline ſalt had conſiderably leſſened the force of the powder. See experiment N° 78.

I next made uſe of *ſal ammoniacum*. That ſalt has been found to produce a very large quantity of gas, or elaſtic vapour, when expoſed to heat, under certain circumſtances; but when 20 grains of it were mixed with a charge of gun-powder, inſtead

ſtead of adding to its force, it diminiſhed it very ſenſibly. See the 80th experiment.

Moſt, if not all, the metals, when they are diſſolved in proper *menſtrua*, cauſe large quantities of gas to be produced or ſet at liberty; and particularly braſs, when it is diſſolved in nitrous acid. Deſirous of ſeeing if this could be done by the flame, or acid vapour of fired gun-powder, I mixed 20 grains of braſs in a very fine powder, commonly called braſs duſt (being the ſmall particles of this metal that fly off from the wheel in ſharpening pins), with 145 grains of powder, and with this compound and a fit bullet I loaded my barrel, and diſcharged it; but the experiment (N° 81.) ſhewed, that the force of the powder was not increaſed by the addition of the braſs duſt, but the contrary.

It ſeems probable, however, that neither braſs duſt nor æthiops mineral are of themſelves capable of diminiſhing the force of gun-powder in any conſiderable degree, otherwiſe than by filling up the interſtices between the grains, and obſtructing the paſſage of the flame, and ſo impeding the progreſs of the inflammation. And hence it appears, how earthy particles and impurities of all kinds are ſo very detrimental to gun-powder. It is not that they deſtroy, or alter the properties of any of the bodies of which the powder is compoſed, but ſimply by obſtructing the progreſs of the inflammation, that they leſſen its force, and render it of little or no value. Too much care, therefore, cannot be taken in manufacturing gun-

powder, to free the materials from all heterogeneous matter.

Of an attempt to ſhoot flame, inſtead of bullets.

Having often obſerved paper and other light bodies to come out of great guns and ſmall arms inflamed, I was led to try if other inflammable bodies might not be ſet on fire in like manner, and particularly inflammable fluids; and I thought if this could be affected, it might be poſſible to project ſuch ignited bodies by the force of the exploſion, and by that means communicate the fire to other bodies at ſome conſiderable diſtance; but in this attempt I failed totally. I never could ſet dry tow on fire at the diſtance of five yards from the muzzle of my barrel. I repeatedly diſcharged large wads of tow and paper, thoroughly ſoaked in the moſt inflammable fluids, ſuch as *alkohol*,—*etherial ſpirit of turpentine*,—*balſam of ſulphur* &c., but none of them were ever ſet, on fire by the exploſion. Sometimes I diſcharged three or four ſpoonfuls of the inflammable fluid, by interpoſing a very thin wad of cork over the powder, and another over the fluid, but ſtill with no better ſucceſs. The fluid was projected againſt the wall, as before, and left a mark where it hit it, but it never could be made to take fire; ſo I gave up the attempt.

EXPERIMENTS

TO DETERMINE

THE FORCE OF FIRED GUN-POWDER.

[Read before the Royal Society, May 4, 1797.]

NO human invention of which we have any authentic records, except, perhaps, the art of printing, has produced ſuch important changes in civil ſociety as the invention of *gun-powder*. Yet, notwithſtanding the uſes to which this wonderful agent is applied are ſo extenſive, and though its operations are as ſurpriſing as they are important, it ſeems not to have hitherto been examined with that care and perſeverance which it deſerves. The exploſion of gun-powder is certainly one of the moſt ſurpriſing phænomena we are acquainted with, and I am perſuaded it would much oftener have been the ſubject of the inveſtigations of ſpeculative philoſophers, as well as of profeſſional men, in this age of inquiry, were it not for the danger attending the experiments: but the force of gun-powder is ſo great, and its effects ſo ſudden and ſo terrible, that, notwithſtanding all the precautions poſſible, there is ever a conſiderable

degree of danger attending the management of it, as I have more than once found to my coſt.

Several eminent philoſophers and mathematicians, it is true, have, from time to time, employed their attention upon this curious ſubject; and the modern improvements in chemiſtry have given us a conſiderable inſight into the cauſe, and the nature of the exploſion which takes place in the inflammation of gun-powder; and the nature and properties of the elaſtic fluids generated in its combuſtion. But the great deſideratum,—the real meaſure of the initial expanſive force of inflamed gun-powder,—ſo far from being known, has hitherto been rather gueſſed at than determined; and no argument can be more convincing to ſhow our total ignorance upon that ſubject, than the difference in the opinions of the greateſt mathematicians of the age, who have undertaken its inveſtigation.

The ingenious Mr. ROBINS, who made a great number of very curious experiments upon gun-powder, and who, I believe, has done more towards perfecting the art of gunnery than any other individual, concluded, as the reſult of all his inquiries and computations, that the force of the elaſtic fluid generated in the combuſtion of gun-powder is 1000 times greater than the mean preſſure of the atmoſphere. But the celebrated mathematician DANIEL BERNOUILLI determines its force to be not leſs than 10,000 times that preſſure, or ten times greater than Mr. ROBINS made it.

Struck with this great difference in the reſults

of

of the computations of theſe two able mathematicians, as well as with the ſubject itſelf, which appeared to me to be both curious and important, I many years ago ſet about making experiments upon gunpowder, with a view principally of determining the point in queſtion, namely, *its initial expanſive force* when fired; and I have ever ſince, occaſionally, from time to time, as I have found leiſure and convenient opportunities, continued theſe inquiries.

In a paper printed in the year 1781, in the LXXI. Volume of the Philoſophical Tranſactions, I gave an account of an experiment (N° 92.) by which it appeared that, calculating even upon Mr. Robins's own principles, the force of gun-powder, inſtead of being 1000 times, muſt at leaſt be 1308 times greater than the mean preſſure of the atmoſphere. However, not only that experiment, but many others, mentioned in the ſame paper, had given me abundant reaſon to conclude that the principles aſſumed by Mr. Robins, in his treatiſe upon gunnery, were erroneous; and I ſaw no poſſibility of ever being able to determine the initial force of gunpowder by the methods he had propoſed, and which I had till then followed in my experiments. Unwilling to abandon a purſuit which had already coſt me much pains, I came to a reſolution to ſtrike out a new road, and to endeavour to aſcertain the force of gun-powder by *actual meaſurement*, in a direct and deciſive experiment.

I ſhall not here give a detail of the numerous difficulties and diſappointments I met with in the

courſe of theſe dangerous purſuits; it will be ſufficient briefly to mention the plan of operations I formed, in order to obtain the end I propoſed, and to give a curſory view of the train of unſucceſsful experiments by which I was at length led to the diſcovery of the truly aſtoniſhing force of gun-powder;—a force at leaſt *fifty thouſand* times greater than the mean preſſure of the atmoſphere!

My firſt attempts were to fire gun-powder in a confined ſpace, thinking, that when I had accompliſhed this, I ſhould find means, without much difficulty, to meaſure its elaſtic force. To this end, I cauſed a ſhort gun-barrel to be made, of the beſt wrought iron, and of uncommon ſtrength; the diameter of its bore was $\frac{3}{4}$ of an inch, its length 5 inches, and the thickneſs of the metal was equal to the diameter of the bore, ſo that its external diameter was $2\frac{1}{4}$ inches. It was cloſed at both ends by two long ſcrews, like the breech-pin of a muſket; each of which entered two inches into the bore, leaving only a vacuity of 1 inch in length for the charge. The powder was introduced into this cavity by taking out one of the ſcrews or breech-pins; which being afterwards ſcrewed into its place again, and both ends of the barrel cloſed up, fire was communicated to the powder by a very narrow vent, made in the axis of one of the breech-pins, for that purpoſe. The chamber, which was 1 inch in length, and $\frac{3}{4}$ of an inch in diameter, being about half filled with powder, I expected that when the powder ſhould be fired,

the

the generated elaſtic fluid being obliged to iſſue out at ſo ſmall an opening as the vent, which was no more than $\frac{1}{20}$ of an inch in diameter, inſtead of giving a ſmart report, would come out with ſomething like a hiſſing noiſe; and I intended, in a future experiment, to confine the generated elaſtic fluid entirely, by adding a valve to the vent, as I had done in ſome of my experiments mentioned in the preceding Paper. But when I ſet fire to the charge (which I took the precaution to do by means of a train), inſtead of a hiſſing noiſe, I was ſurpriſed by a very ſharp and a very loud report; and, upon examining the barrel, I found the vent augmented to at leaſt four times its former dimenſions, and both the ſcrews looſened.

Finding, by the reſult of this experiment, that I had to do with an agent much more troubleſome to manage than I had imagined, I redoubled my precautions. As the barrel was not eſſentially injured, its ends were now cloſed up by two new ſcrews, which were firmly fixed in their places by ſolder, and a new vent was opened in the barrel itſelf. As both ends of the barrel were now cloſed up, it was neceſſary, in order to introduce the powder into the chamber, to make it paſs through the vent, or to convey it through ſome other aperture made for that purpoſe. The method I employed was as follows: a hole being made in the barrel, about $\frac{2}{10}$ of an inch in diameter, a plug of ſteel was ſcrewed into this hole; and it was in the centre or axis of the plug that the vent was made. To introduce the powder into the chamber the plug

was taken away. The vent was made conical, its largeſt diameter being inwards, or opening into the chamber; and a conical pin, of hardened ſteel, was fitted into it; which pin was intended to ſerve as a valve for cloſing up the vent, as ſoon as the powder in the chamber ſhould be inflamed. To give a paſſage to the fire through the vent in entering the chamber, this pin was puſhed a little inwards, ſo as to leave a ſmall vacuity between its ſurface and the concave ſurface of the bore of the vent.

But notwithſtanding all poſſible care was taken in the conſtruction of this inſtrument to render it perfect in all its parts, the experiment was as unſucceſsful as the former: upon firing the powder in the chamber, (though it did not fill more than half its cavity), the generated elaſtic fluid not only forced its way through the vent, notwithſtanding the valve (which appeared not to have had time to cloſe), but it iſſued with ſuch an aſtoniſhing velocity from this ſmall aperture, that inſtead of coming out with a hiſſing noiſe, it gave a report nearly as ſharp and as loud as a common muſket. Upon examining the vent-plug and the pin, they were both found to be much corroded and damaged; though I had taken the precaution to harden them both before I made the experiment.

I afterwards repeated the experiment with a ſimple vent, made very narrow, and lined with gold to prevent its being corroded by the acid vapour generated in the combuſtion of the gun-powder; but this vent was found, upon trial, to be as little

able

able to withſtand the amazing force of the inflamed gun-powder as the others. It was ſo much, and ſo irregularly corroded, by the exploſion, in the firſt experiment, as to be rendered quite unſerviceable; and what is ſtill more extraordinary, the barrel itſelf, notwithſtanding its amazing ſtrength, was blown out into the form of a caſk; and though it was cracked, it was not burſt quite aſunder, nor did it appear that any of the generated elaſtic fluid had eſcaped through the crack. The barrel, in the ſtate it was found after this experiment, is ſtill in my poſſeſſion.

Theſe unſucceſsful attempts, and many others of a ſimilar nature, of which it is not neceſſary to give a particular account, as they all tended to ſhew that the force of fired gun-powder is in fact much greater than has generally been imagined, inſtead of diſcouraging me from purſuing theſe inquiries, ſerved only to excite my curioſity ſtill more, and to ſtimulate me to further exertions.

Theſe reſearches did not by any means appear to me as being merely ſpeculative; on the contrary, I conſidered the determination of the real force of the elaſtic fluid generated in the combuſtion of gun-powder as a matter of great importance.

The uſe of gun-powder is become ſo extenſive, that very important mechanical improvements can hardly fail to reſult from any new diſcoveries relative to its force, and the law of its action. Moſt of the computations that have hitherto been made relative to the action of gun-powder, have been founded upon the ſuppoſition that the elaſticity of the generated

generated fluid is, in all cafes, as its denfity; but if this fuppofition fhould prove falfe, all thofe computations, with all the practical rules founded on them, muft neceffarily be erroneous; and the influence of thefe errors muft be as extenfive as the ufes to which gun-powder is applied.

Having found by experience how difficult it is to confine the elaftic vapour generated in the combuftion of gun-powder, when the fmalleft opening is left by which any part of it can efcape, it occurred to me, that I might perhaps fucceed better by clofing up the powder entirely, in fuch a manner as to leave no opening whatever, by which it could communicate with the external air; and by fetting the powder on fire, by caufing the heat employed for that purpofe to pafs through the folid fubftance of the iron barrel ufed for confining it. In order to make this experiment, I caufed a new barrel to be conftructed for that purpofe: its length was 3.45 inches, and the diameter of its bore $\frac{7}{10}$ of an inch; its ends were clofed up by two fcrews, each 1 inch in length, which were firmly and immoveably fixed in their places by folder; a vacuity being left between them in the barrel 1.45 inch in length, which conftituted the chamber of the piece; and whofe capacity was nearly $\frac{6}{10}$ of a cubic inch. An hole, 0.37 of an inch in diameter, being bored through both fides of the barrel, through the center of the chamber, and at right angles to its axis, two tubes of iron, 0.37 of an inch in diameter, the diameter of whofe bore was $\frac{1}{10}$ of an inch, were firmly fixed in this hole with folder, in fuch a man-

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ner that while their internal openings were exactly oppoſite to each other, and on oppoſite ſides of the chamber, the axis of their bores were in the ſame right line. The ſhorteſt of theſe tubes, which projected 1.3 inch beyond the external ſurface of the barrel, was cloſed at its projecting end, or rather it was not bored quite through its whole length, $\frac{3}{10}$ of an inch of ſolid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the ſurface of the barrel, on the other ſide, and which ſerved for introducing the powder into the chamber, was open; but it could occaſionally be cloſed by a ſtrong ſcrew, furniſhed with a collar of oiled leather, which was provided for that purpoſe. The method of making uſe of this inſtrument was as follows. The barrel being laid down, or held, in an horizontal poſition, with the long tube upwards, the charge, which was of the very beſt fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care muſt be taken that the cavity of the ſhort tube be completely filled with powder, and this can beſt be done by pouring in only a ſmall quantity of powder at firſt, and then, by ſtriking the barrel with a hammer, cauſe the powder to deſcend into the ſhort tube. When, by introducing a priming-wire through the long tube, it is found that the ſhort tube is full, it ought to be gently preſſed together, or rammed down, by means of the priming-wire, in order to prevent its falling back into the chamber upon moving the barrel out of the horizontal poſition. The ſhort tube being

properly

properly filled, the reſt of the charge may be introduced into the chamber, and the end of the long tube cloſed up by its ſcrew.

More effectually to prevent the elaſtic fluid generated in the combuſtion of the charge from finding a paſſage to eſcape by this opening, after the charge was introduced into the chamber, the cavity of the long tube was filled up with cold tallow, and the ſcrew that cloſed up its end (which was $\frac{1}{2}$ an inch long, and but a little more than $\frac{1}{10}$ of an inch in diameter) was preſſed down againſt its leather collar with the utmoſt force. The manner of ſetting fire to the charge was as follows: a block of wrought iron, about at $1\frac{1}{2}$ inch ſquare, with a hole in it, capable of receiving nearly the whole of that part of the *ſhort tube* which projects beyond the barrel, being heated red hot, the end of the ſhort tube was introduced into this hole, where it was ſuffered to remain till the heat, having penetrated the tube, ſet fire to the powder it contained, and the inflammation was *from thence* communicated to the powder in the chamber.

The reſult of this experiment fully anſwered my expectations. The generated elaſtic fluid was ſo completely confined that no part of it could make its eſcape. The report of the exploſion was ſo very feeble, as hardly to be audible: indeed it did not by any means deſerve the name of a report, and certainly could not have been heard at the diſtance of twenty paces; it reſembled the noiſe which is occaſioned by the breaking of a very ſmall glaſs tube.

I imagined at firſt that the powder had not all

taken

taken fire, but the heat of the barrel ſoon convinced me that the exploſion muſt have taken place, and after waiting near half an hour, upon looſening the ſcrew which cloſed the end of the long vent-tube, the confined elaſtic vapours ruſhed out with conſiderable force, and with a noiſe like that attending the diſcharge of an air-gun. The quantity of powder made uſe of in the experiment was indeed very ſmall, not amounting to more than $\frac{1}{5}$ part of what the chamber was capable of containing; but having ſo often had my machinery deſtroyed in experiments of this ſort, I began now to be more cautious.

Having found means to confine the elaſtic vapour generated in the combuſtion of gun-powder, my next attempts were, to meaſure its force; but here again I met with new and almoſt inſurmountable difficulties. To meaſure the expanſive force of the vapour, it was neceſſary to bring it to act upon a moveable body of known dimenſions, and whoſe reſiſtance to the efforts of the fluid could be accurately determined; but this was found to be extremely difficult. I attempted it in various ways, but without ſucceſs. I cauſed a hole to be bored in the axis of one of the ſcrews, or breech-pins, which cloſed up the ends of the barrel juſt deſcribed, and fitting a piſton of hardened ſteel into this hole (which was $\frac{2}{10}$ of an inch in diameter), and cauſing the end of the piſton which projected beyond the end of the barrel to act upon a heavy weight, ſuſpended as a pendulum to a long iron rod, I hoped, by knowing the velocity acquired by the weight, from the length of the arc deſcribed by it in its aſcent, to be able to calculate

calculate the preſſure of the elaſtic vapour by which it was put in motion; but this contrivance was not found to anſwer, nor did any of the various alterations and improvements I afterwards made in the machinery render the reſults of the experiment at all ſatisfactory. It was not only found almoſt impoſſible to prevent the eſcape of the elaſtic fluid by the ſides of the piſton, but the reſults of apparently ſimilar experiments were ſo very different, and ſo uncertain, that I was often totally at a loſs to account for theſe extraordinary variations. I was however at length led to ſuſpect, what I afterwards found abundant reaſon to conclude was the real cauſe of theſe variations, and of all the principal difficulties which attended the aſcertaining the force of fired gun-powder by the methods I had hitherto purſued.

It has generally been believed, after Mr. ROBINS, that the force of fired gun-powder conſiſts in the action of a permanently elaſtic fluid, ſimilar in many reſpects to common atmoſpheric air; which, being generated from the powder in combuſtion, in great abundance, and being moreover in a very compreſſed ſtate, and its elaſticity being much augmented by the heat (which is likewiſe generated in the combuſtion), it eſcapes with great violence by every avenue, and produces that loud report, and all thoſe terrible effects, which attend the exploſion of gun-powder.

But though this theory is very plauſible, and ſeems upon a curſory view of the ſubject to account in a ſatisfactory manner for all the phænomena, yet a more careful examination will ſhew it to be defective. There is no doubt but the permanently elaſtic fluids,

generated in the combuſtion of gun-powder, *aſſiſt* in producing thoſe effects which reſult from its exploſion; but it will be found, I believe, upon aſcertaining the real expanſive force of fired gun-powder, that this cauſe alone is quite inadequate to the effects actually produced; and that, therefore, the agency of ſome other power muſt neceſſarily be called in to its aſſiſtance.

Mr. Robins has ſhewn, that if all the permanently elaſtic fluid generated in the combuſtion of gun-powder be compreſſed in the ſpace originally occupied by the powder, and if this fluid, ſo compreſſed, be ſuppoſed to be heated to the intenſe heat of red-hot iron, its elaſtic force *in that caſe* will be 1000 times greater than the mean preſſure of the atmoſphere; and this, according to his theory, is the real meaſure of the force of gun-powder *fired in a cavity which it exactly fills.*

But what will become of this theory, and of all the ſuppoſitions upon which it is founded, if I ſhall be able to prove, as I hope to do in the moſt ſatisfactory manner, that the force of fired gun-powder, inſtead of being 1000 times, is at leaſt 50,000 greater than the mean preſſure of the atmoſphere?

For my part, I know of no way of accounting for this enormous force, but by ſuppoſing it to ariſe principally from the elaſticity of the *aqueous vapour* generated from the powder in its combuſtion. The brilliant diſcoveries of modern chemiſts have taught us, that both the conſtituent parts of which water is compoſed, and even water itſelf, exiſt in the materials which are combined to make gun-powder; and

and there is much reaſon to believe that aqueous vapour, or ſteam, is actually formed, in its combuſtion. M. LAVOISIER, I know, imagined that the force of fired gun-powder depends in a great meaſure upon the expanſive force of uncombined *caloric*, ſuppoſed to be let looſe in great abundance during the combuſtion or deflagration of the powder: but it is not only dangerous to admit the action of an agent whoſe *exiſtence* is not yet clearly demonſtrated, but it appears to me that this ſuppoſition is quite unneceſſary; the elaſtic force of the heated aqueous vapour, whoſe exiſtence can hardly be doubted, being quite ſufficient to account for all the phænomena.

It is well known that the elaſticity of aqueous vapour is conſiderably more augmented by any given augmentation of temperature, than that of any permanently elaſtic fluid whatever; and thoſe who are acquainted with the amazing force of ſteam, when heated only to a few degrees above the boiling point, can eaſily perceive that its elaſticity muſt be almoſt infinite, when greatly condenſed and heated to the temperature of red-hot iron: and this heat it muſt certainly acquire in the exploſion of gun-powder. But if the force of fired gun-powder ariſes *principally* from the elaſtic force of heated aqueous vapour, a cannon is nothing more than a *ſteam-engine* upon a peculiar conſtruction; and upon determining the ratio of the elaſticity of this vapour to its denſity, and to its temperature, a law will be found to obtain, very different from that aſſumed by Mr. ROBINS, in his Treatiſe on Gunnery. What this law really is, I do not pretend to have deter-

mined with that degree of preciſion which I wiſhed; but the experiments of which I am about to give an account will, I think, demonſtrate in the moſt ſatisfactory manner, not only that the force of fired gunpowder is in fact much greater than has been imagined, but alſo that its force conſiſts principally in the temporary action of a fluid not permanently elaſtic, and conſequently that all the theories hitherto propoſed for the elucidation of this ſubject, muſt be eſſentially erroneous.

The firſt ſtep towards acquiring knowledge is undoubtedly that which leads us to a diſcovery of the falſehood of received opinions. To a diligent inquirer every common operation, performed in the uſual courſe of practice, is an experiment, from which he endeavours to diſcover ſome new fact, or to confirm the reſult of former inquiries.

Having been engaged many years in the inveſtigation of the force of gun-powder, I occaſionally found many opportunities of obſerving, under a variety of circumſtances, the various effects produced by its exploſion; and as a long habit of meditating upon this ſubject rendered every thing relating to it highly intereſting to me, I ſeized theſe opportunities with avidity, and examined all the various phænomena with ſteady and indefatigable attention.

During a cruiſe which I made, as a volunteer, in the Victory, with the Britiſh fleet, under the command of my late worthy friend Sir CHARLES HARDY, in the year 1779, I had many opportunities of attending to the firing of heavy cannon: for though we were not fortunate enough to come

to a general action with the enemy, as is well known, yet, as the men were frequently exercised at the great guns, and in firing at marks, and as some of my friends in the fleet, then captains, (since made admirals) as the Honourable KEITH STEWART, who commanded the Berwick of 74 guns—Sir CHARLES DOUGLAS, who commanded the Duke of 98 guns—and Admiral MACBRIDE, who was then captain of the Bienfaisant of 64 guns, were kind enough, at my request, to make a number of experiments, and particularly by firing a greater number of bullets at once from their heavy guns than ever had been done before, and observing the distances at which they fell in the sea; I had opportunities of making several very interesting observations, which gave me much new light relative to the action of fired gun-powder. And afterwards, when I went out to America, to command a regiment of cavalry which I had raised in that country for the King's service, his Majesty having been graciously pleased to permit me to take out with me from England four pieces of light artillery, constructed under the direction of the late Lieutenant General DESAGULIERS, with a large proportion of ammunition, I made a great number of interesting experiments with these guns, and also with the ship guns on board the ships of war in which I made my passage to and from America.

It would take up too much time, and draw out this paper to too great a length, to give an account in detail of all these experiments, and of the various observations I have had opportunities of making

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from time to time, relative to this ſubject. I ſhall, therefore, only obſerve at preſent, that the reſult of all my inquiries tended to confirm me more and more in the opinion, that the theory generally adopted relative to the exploſion of gun-powder was extremely erroneous, and that its force is in fact much greater than is generally imagined. That the poſition of Mr. ROBINS, which ſuppoſes the inflammation and combuſtion of gun-powder to be ſo inſtantaneous "that the whole of the charge of a "piece of ordnance is actually inflamed and con-"verted into an elaſtic vapour before the bullet is "ſenſibly moved from its place," is very far from being true; and that the ratio of the elaſticity of the generated fluid to its denſity, or to the ſpace it occupies as it expands, is very different from that aſſumed by Mr. ROBINS.

The rules laid down by Mr. ROBINS for computing the velocities of bullets from their weight, the known dimenſions of the gun, and the quantities of powder made uſe of for the charge, may, and certainly do, very often give the velocities very near the truth; but this is no proof that the principles upon which theſe computations are made are juſt; for it may eaſily happen, that a complication of erroneous ſuppoſitions may be ſo balanced, that the reſult of a calculation founded on them may, nevertheleſs, be very near the truth; and this is never ſo likely to happen as when, from known effects, the action of the powers which produce them are computed. For it is not in general very difficult to aſſume ſuch principles as, when taken toge-

ther, may in the moſt common known caſes anſwer completely all the conditions required. But in ſuch caſes, if the truth be diſcovered with regard to any one of the aſſumed principles, and it be ſubſtituted in the place of the erroneous ſuppoſition, the fallacy of the whole hypotheſis will immediately become evident.

As I have mentioned the experiments made with heavy artillery, as having been led by their reſults to form important conjectures relative to the nature of the expanſion of the fluid generated in the combuſtion of gun-powder; it may perhaps be aſked, and indeed with ſome appearance of reaſon, what the circumſtances were which attended the experiments in queſtion, which could juſtify ſo important a concluſion as that of the fallacy of the commonly received theory relative to that ſubject. To this I anſwer briefly, that in regard to the ſuppoſed inſtantaneous inflammation of the powder, upon which the whole fabric of this theory is built, or rather of all the computations which are grounded upon it, a careful attention to the phænomena which take place upon firing off cannon, led me to ſuſpect, or rather confirmed me in my former ſuſpicions, that however rapid the inflammation of gun-powder may be, its *total combuſtion* is by no means ſo ſudden as this theory ſuppoſes. When a heavy cannon is fired in the common way, that is, when the vent is filled with looſe powder, and the piece is fired off with a match, the time employed in the paſſage of the inflammation through the vent into the chamber of the piece is perfectly ſenſible,

and

and this time is evidently ſhorter after the piece has been heated by repeated firing. With the ſame charge, the recoil of a gun (and conſequently the velocity of its bullet,) is greater after the gun has been heated by repeated firing than when it is cold. The velocity of the bullet is conſiderably greater when the cannon is fired off with a vent tube, or by firing a piſtol charged with powder into the open vent, than when the vent is filled with looſe powder. The velocity of two, three, or more fit bullets diſcharged at once from a piece of ordnance, compared to the velocity of one ſingle bullet diſcharged by the ſame quantity of powder, from the ſame cannon, is greater than it ought to be according to the theory. Conſiderable quantities of powder are frequently driven out of cannon and other fire-arms *unconſumed*. The manner in which the ſmoke of gun-powder riſes in the air, and is gradually diſſolved and rendered inviſible, ſhews it to partake of the nature of ſteam. But not to take up too much time with theſe general obſervations, I ſhall proceed to give an account of experiments, the reſults of which will be conſidered as more concluſive.

Having found it impoſſible to meaſure the elaſtic force of fired gun-powder with any degree of preciſion by any of the methods before mentioned, I totally changed my plan of operations, and inſtead of endeavouring to determine its force by cauſing the generated elaſtic fluid to act upon a moveable body through a determined ſpace, I ſet about contriving an apparatus in which this fluid ſhould

be made to act, by a determined ſurface, againſt a weight, which, by being increaſed at pleaſure, ſhould at laſt be ſuch as would juſt be able to confine it, and which in that caſe would juſt counterbalance, and conſequently *meaſure* its elaſtic force.

The idea of this method of determining the force of fired gun-powder occurred to me many years ago; but a very expenſive and troub'eſome apparatus being neceſſary in order to put it in execution, it was not till the year 1792, when, being charged with the arrangement of the army of his moſt Serene Highneſs the ELECTOR PALATINE, reigning Duke of Bavaria, and having all the reſources of the military arſenal, and a number of very ingenious workmen at my command, with the permiſſion and approbation of his moſt Serene Electoral Highneſs, I ſet about making the experiments which I ſhall now deſcribe: and as they are not only important in themſelves, and in their reſults, but as they are, I believe, the firſt of the kind that have been made, I ſhall be very particular in my account of them, and alſo of the apparatus uſed in making them.

One difficulty being got over, that of ſetting fire to the powder without any communication with the external air, by cauſing the heat employed for that purpoſe to paſs through the ſolid ſubſtance of the barrel; it only remained to apply ſuch a weight to an opening made in the barrel as the whole force of the generated elaſtic fluid ſhould not be able to lift, or diſplace; but in doing this many precautions were neceſſary. For, firſt, as the force of gun-powder is ſo very great, it was neceſſary to

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employ an enormous weight to confine it; for, although by diminiſhing the ſize of the opening, the weight would be leſſened in the ſame proportion, yet it was neceſſary to make this opening of a certain ſize, otherwiſe the experiments would not have been ſatisfactory; and it was neceſſary to make the ſupport or baſe upon which the barrel was placed very maſſy and ſolid, to prevent the errors which would unavoidably have ariſen from its want of ſolidity, or from its elaſtity.

The annexed drawings will give a complete idea of the whole apparatus made uſe of in theſe experiments. A (fig. 1.) is a ſolid block of very hard ſtone, 4 feet 4 inches ſquare, placed upon a bed of ſolid maſonry, which deſcended 6 feet below the ſurface of the earth. Upon this block of ſtone, which ſerved as a baſe to the whole machinery, was placed the barrel B of hammered iron, upon its ſupport C, which is of caſt braſs, or rather of gun-metal; which ſupport was again placed upon a circular plate of hammered iron D, 8 inches in diameter, and $\frac{3}{4}$ of an inch thick, which laſt reſted upon the block of ſtone. The opening of the bore of the barrel (which was placed in a vertical poſition, and which was juſt $\frac{1}{4}$ of an inch in diameter), was cloſed by a ſolid hemiſphere E of hardened ſteel, whoſe diameter was 1.16 inch; and upon this hemiſphere the weight F, made uſe of for confining the elaſtic fluid generated from the powder in its combuſtion, repoſed. This weight (which in ſome of the moſt intereſting experiments was a braſs cannon, a heavy twenty-four pounder,

placed vertically upon its cafcabel), being fixed to the timbers G G which formed a kind of carriage for it, was moveable up and down; the ends of theſe timbers being moveable in grooves cut in the vertical timbers K K, which being fixed below in holes made to receive them in the block of ſtone, and above by a croſs piece L, were ſupported by braces and iron clamps made faſt to the thick walls of the building of the arſenal. This weight was occaſionally raiſed and lowered in the courſe of the experiments (in placing and removing the barrel), by means of a very ſtrong lever, which is omitted in the drawing to make it leſs complicated. The barrel, a ſection of which is repreſented in fig. 2. of its natural ſize, is 2.78 inches long, and 2.82 inches in diameter, at its lower extremity, where it repoſes upon its ſupporter, but ſomething leſs above, being ſomewhat diminiſhed, and rounded off at its upper extremity. Its bore, which, as I have already obſerved, is $\frac{1}{4}$ of an inch in diameter, is 2.13 inches long, and it ends in a very narrow opening below, not more than 0.07 of an inch in diameter, and 1.715 inch long, which forms the vent (if I may be permitted to apply that name to a paſſage which is not open at both ends), by which the fire is communicated to the charge. From the centre of the bottom of the barrel there is a projection of about 0.45 of an inch in diameter, and 1.3 inch long, which forms the vent tube V. Fig. 3. is a view of an iron ball W, which, being heated red-hot, and being applied to the vent tube by means of an hole O, made in it for that purpoſe, fire is communicated

through

through the ſolid ſubſtance of the vent tube to the powder it contains, and from thence to the charge.

Fig. 4. which is drawn on a ſcale of two inches to the inch, or half the real ſize of the machinery, ſhews how the barrel B, was placed upon its ſupport C; how this laſt was placed upon its circular plate of iron D, and how the red-hot iron ball W, was applied to the vent tube V. This ball is managed by means of a long handle *h*, of iron, and being introduced through a circular opening *g*, in the ſupport, and applied to the vent tube V, is kept in its place by means of a wedge, or rather lever *l*, whoſe external end is repreſented in the drawing as being broken off, to ſave room. The circular opening in front of the ſupport is ſeen in front, and conſequently more diſtinctly, in the drawing, fig. 1. In this drawing the end of the vent tube may be likewiſe diſcovered through this opening; but as it was neceſſary, in order to introduce all the parts of this machinery, to make the drawing upon a very ſmall ſcale, it was not poſſible to expreſs all the ſmaller parts with that diſtinctneſs which I wiſhed. The other figures which are added, in which the parts are expreſſed ſeparately, and upon a larger ſcale, will, it is hoped, ſupply this defect.

The ſtand, or ſupport, as I have called it, upon which the barrel was placed, is circular, and in order that it might be united more firmly to the plate of iron upon which it repoſes, this plate is furniſhed with a cylindrical projection *p*, 1 inch long and 1 $\frac{1}{2}$ in diameter, which enters a hole made in the bottom of the ſtand to receive it.

Fig.

Fig. 5. is a view of the barrel from above, in which the projecting ſcrews, or rather cylinders, are ſeen, by which the hemiſphere E, fig. 2. which cloſed the end of the barrel, was kept in its place. Two of theſe ſcrews 1, 2, are ſeen in the figures 2, and 4. The ſmaller circle *a b*, fig. 5. ſhews the diameter of a circular plate of gold, which was let into the end of the barrel, being firmly fixed to the iron with ſolder; and the larger circle *c d* repreſents a circular piece of oiled leather, which was placed between the end of the barrel and the hemiſphere which reſted upon it.

The end of the barrel was covered with gold, in order to prevent, as much as poſſible, its being corroded by the elaſtic vapour, which, when the weight is not heavy enough to confine it, eſcapes between the end of the barrel and the flat ſurface of the hemiſphere; but even this precaution was not found to be ſufficient to defend the apparatus from injury. The ſharp edge of the barrel, at the mouth of the bore, was worn away almoſt immediately, and even the flat ſurface of the hemiſphere, notwithſtanding it was of hardened ſteel and very highly poliſhed, was ſenſibly corroded. This coroſion of the mouth of the bore, by which the dimenſions of the ſurface upon which the generated elaſtic fluid acted were rendered very uncertain, would alone have been ſufficient to have rendered all my attempts to determine the force of fired gunpowder abortive, had I not found means to remedy the evil. The method I employed for this purpoſe was as follows. Having provided ſome pieces of

very

very good compact ſole-leather, I cauſed them to be beaten upon an anvil with a heavy hammer, to render them ſtill more compact; and then, by means of a machine made for that purpoſe, cylindric ſtoppers, of the ſame diameter preciſely as the bore of the barrel, and 0.13 of an inch in length (that is to ſay, equal in length to the thickneſs of the leather), were formed of it; and one of theſe ſtoppers, which had previouſly been greaſed with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the exploſion taking place, this ſtopper (being preſſed on the one ſide by the generated elaſtic fluid, and on the other by the hemiſphere, loaded with the whole weight employed to confine the powder), ſo completely cloſed the bore, that, when the force of the powder was not ſufficient to raiſe the weight to ſuch a height that the ſtopper was actually blown out of the piece, not a particle of the elaſtic fluid could make its eſcape. And in thoſe caſes in which the weight was actually raiſed, and the generated elaſtic fluid made its eſcape, as it did not corrode the barrel in any other part but juſt *at the very extremity of the bore*, the experiment by which the weight was aſcertained, which was juſt able to counterbalance the preſſure of the generated elaſtic fluid, was in nowiſe vitiated, either by the increaſed diameter of the bore at its extremity, or by any corroſion of the hemiſphere itſelf; for as long as the bore retained its form and its dimenſions, in that part to which the efforts of the elaſtic fluid

were

were confined, that is, in that part of the bore immediately in contact with the lower part of the ſtopper, the experiment could not be affected by any imperfection of the bore either above or below.

In the figures 2. and 4. this ſtopper is repreſented in its place, and fig. 6. ſhews the plan, and fig. 7. the profile of one of theſe ſtoppers of its full ſize. Fig. 8. ſhews a ſmall, but very uſeful inſtrument, employed in introducing theſe ſtoppers into the bore, and more eſpecially in occaſionally extracting them: it reſembles a common cork-ſcrew, only it is much ſmaller. In the figure (where it is ſhewn in its full ſize), it is repreſented ſcrewed into a ſtopper. Fig. 9. ſhews the plan, and fig. 10. a ſide view, of the full ſize, of the hemiſphere of hardened ſteel, by which the end of the barrel was cloſed. In the figures 2. and 4. the barrel is repreſented as being about half filled with powder.

Preſuming that what has been already ſaid, together with the aſſiſtance of the annexed drawings, will be ſufficient to give a perfect idea of all the different parts of this apparatus, I ſhall now proceed to give an account of the experiments, which, from time to time have been made with it. And in order to render theſe details as intelligible as poſſible, and to ſhew the reſults of all theſe enquiries in a clear and ſatisfactory manner, I ſhall firſt give a brief account of the manner in which the experiments were made; of the various precautions uſed; and the particular appearances which were obſerved in the proſecution of them.

The powder made uſe of in theſe experiments

was

was of the beſt quality, being that kind called *poudre de chaſſe* by the French, and very fine grained: and it was all taken from the ſame parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for uſe was very dry. The weights employed for weighing the powder were German apothecary's grains, 104.8 of which make 100 grains Troy. I have reduced the weights employed to confine the elaſtic vapour generated in the combuſtion of the powder from Bavarian pounds, in which they were originally expreſſed, to pounds avoirdupois. The meaſures of length were all taken in Engliſh feet and inches. The experiments were all made in the open air, in the court-yard of the arſenal at Munich; and they were all made in fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore cloſed by its leather ſtopper, in the room where the powder was weighed. In placing the barrel upon the block of ſtone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elaſtic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever which ſupported it, the exploſion very ſoon followed.

When the force of the generated elaſtic vapour was ſufficient to raiſe the weight, the exploſion was attended by a very ſharp and ſurpriſingly loud report; but when the weight was not raiſed; as alſo when it was only a little moved, but not ſuffi-

ciently

ciently to permit the leather ſtopper to be driven quite out of the bore, and the elaſtic fluid to make its eſcape, the report was ſcarcely audible at the diſtance of a few paces, and did not at all reſemble the report which commonly attends the exploſion of gun-powder. It was more like the noiſe which attends the breaking of a ſmall glaſs tube than any thing elſe to which I can compare it. In many of the experiments in which the elaſtic vapour was confined, this feeble report attending the exploſion of the powder was immediately followed by another noiſe, totally different from it, which appeared to be occaſioned by the falling back of the weight upon the end of the barrel, after it had been a little raiſed, but not ſufficiently to permit the leather ſtopper to be driven quite out of the bore. In ſome of theſe experiments, a very ſmall part only of the generated elaſtic fluid made its eſcape: in theſe caſes the report was of a peculiar kind, and though perfectly audible at ſome conſiderable diſtance, yet not at all reſembling the report of a muſket. It was rather a very ſtrong, ſudden hiſſing, than a clear, diſtinct, and ſharp report.

Though it could be determined with the utmoſt certainty by the report of the exploſion, whether any part of the generated elaſtic fluid had made its eſcape, yet for ſtill greater precaution, a light collar of very clean cotton wool was placed round the edge of the ſteel hemiſphere, where it repoſed upon the end of the barrel, which could not fail to indicate, by the black colour it acquired, the eſcape of the elaſtic fluid, whenever it was ſtrong enough

to raiſe the weight by which it was confined, ſufficiently to force its way out of the barrel.

Though the end of the barrel, at the mouth of the bore, was covered with a circular plate of gold, in order the better to defend the mouth of the bore againſt the effects of the corroſive vapour, yet this plate being damaged in the courſe of the experiments (a piece of it being blown away), the remainder of it was removed; and it was never after thought neceſſary to replace it by another. When this plate of gold was taken away, the length of the barrel was of courſe diminiſhed as much as the thickneſs of this plate amounted to, which was about $\frac{1}{400}$ part of an inch; but in order that even this ſmall diminution of the length of the barrel might have no effect on the reſult of the experiments, its bore was deepened $\frac{1}{400}$ of an inch when this plate was removed, ſo that the capacity of the bore remained the ſame as before.

After making uſe of a great variety of expedients, the beſt and moſt convenient method of cloſing the end of the bore, and defending the flat ſurface of the ſteel hemiſphere from the corroding vapours, was found to be this; firſt, to cover the end of the bore with a circular plate of thin oiled leather, then to lay upon this a very thin circular plate of hammered braſs, and upon this braſs plate the flat ſurface of the hemiſphere. When the elaſtic fluid made its eſcape, a part of the leather was conſtantly found to have been torn away, but never in more places than one; that is to ſay, always on one ſide only.

What

What was very remarkable in all thoſe experiments in which the generated elaſtic vapour was completely confined, was the ſmall degree of expanſive force which this vapour appeared to poſſeſs after it had been ſuffered to remain a few minutes, or even only a few ſeconds, confined in the barrel; for, upon raiſing the weight by means of its lever, and ſuffering this vapour to eſcape, inſtead of eſcaping with a loud report, it ruſhed out with a hiſſing noiſe hardly ſo loud or ſo ſharp as the report of a common air gun; and its efforts againſt the leathern ſtopper, by which it aſſiſted in raiſing the weight, were ſo very feeble as not to be ſenſible. Upon examining the barrel, however, this diminution of the force of the generated elaſtic fluid was eaſily explained; for what was undoubtedly in the moment of the exploſion in the form of an elaſtic fluid, was now found transformed into a *ſolid body* as hard as a ſtone! It may eaſily be imagined how much this unexpected appearance excited my curioſity; but, intent on the proſecution of the main deſign of theſe experiments, the aſcertaining the force of fired gun-powder, I was determined not to permit myſelf to be enticed away from it by any extraordinary or unexpected appearances, or accidental diſcoveries, however alluring they might be; and faithful to this reſolution, I poſtponed the examination of this curious phænomenon to a future period; and ſince that time I have not found leiſure to engage in it. I think it right, however, to mention in this place ſuch curſory obſervations as I was able, in the midſt of my other purſuits, to make

make upon this ſubject; and it will afford me ſincere pleaſure, if what I have to offer ſhould ſo far excite the curioſity of philoſophers, as to induce ſome one who has leiſure, and the means of purſuing ſuch inquiries with effect, to precede me in the inveſtigation of this intereſting phænomenon; and as the ſubject is certainly not only extremely curious in itſelf, but bids fair to lead to other and very important diſcoveries, I cannot help flattering myſelf that ſome attention will be paid to it.

I have ſaid that the ſolid ſubſtance into which the elaſtic vapour generated in the combuſtion of gunpowder was transformed, was *as hard as a ſtone*. This I am ſenſible is but a vague expreſſion; but the fact is, that it was very hard, and ſo firmly attached to the inſide of the barrel, and particularly to the inſide of the upper part of the vent tube, that it was always neceſſary, in order to remove it, to make uſe of a drill, and frequently to apply a conſiderable degree of force. This ſubſtance, which was of a black colour, or rather of a dirty grey, which changed to black upon being expoſed to the air, had a pungent, acrid, alkaline taſte, and ſmelt like liver of ſulphur. It attracted moiſture from the air with great avidity. Being moiſtened with water, and ſpirit of nitre being poured upon it, a ſtrong effervеſcence enſued, attended by a very offenſive and penetrating ſmell. Nearly the whole quantity of matter of which the powder was compoſed, ſeemed to have been transformed into this ſubſtance; for the quantity of elaſtic fluid which eſcaped upon removing the weight, was very inconſiderable;

ſiderable; but this ſubſtance was *no longer gun-powder;* it was not even inflammable. What change had it undergone? what could it have loſt? It is very certain that the barrel was conſiderably heated in theſe experiments. Was this occaſioned by the *caloric*, diſengaged from the powder in its combuſtion, making its eſcape through the iron? And is this a proof of the exiſtence of *caloric*, conſidered as a fluid *ſui generis;* and that it actually enters into the compoſition of inflammable bodies, or of pure air, and is neceſſary to their combuſtion? I dare not take upon me to decide upon ſuch important queſtions. I once thought that the heat acquired by a piece of ordnance in being fired, aroſe from the vibration or friction of its parts, occaſioned by the violent blow it received in the exploſion of the powder; but I acknowledge fairly, that it does not ſeem to be poſſible to account in a ſatisfactory manner for the very conſiderable degree of heat which the barrel acquired in theſe experiments, merely on that ſuppoſition.

That this hard ſubſtance, found in the barrel after an experiment in which the generated elaſtic vapour had been completely confined, was actually in a fluid or elaſtic ſtate in the moment of the exploſion, is evident from hence, that in all thoſe caſes in which the weight was raiſed, and the ſtopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable that this hard ſubſtance was not found diſtributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the

bore, than at either of its extremities; and the upper part of the vent tube in particular was always found quite filled with it. It ſhould ſeem from hence, that it attached itſelf to thoſe parts of the barrel which were ſooneſt cooled; and hence the reaſon, moſt probably, why none of it was ever found in the lower part of the vent tube, where it was kept hot by the red-hot ball by which the powder was ſet on fire.

I found by a particular experiment, that the gunpowder made uſe of, when it was well ſhaken together, occupied rather leſs ſpace in any given meaſure, than the ſame weight of water; conſequently when gunpowder is fired in a confined ſpace which it fills, the denſity of the generated elaſtic fluid muſt be at leaſt equal to the denſity of water. The real ſpecific gravity of the ſolid grains of gun-powder, determined by weighing them in air, and in water, is to the ſpecific gravity of water, as 1.868 to 1.000. But if a meaſure, whoſe capacity is one cubic foot, hold 1000 ounces of water, the ſame meaſure will hold juſt 1077 ounces of fine grained gun-powder, ſuch as I made uſe of in my experiments; that is to ſay, when it is well ſhaken together. When it was moderately ſhaken together, I found its weight to be exactly equal to that of an equal volume, or rather meaſure, of water. But it is evident that the weight of any given meaſure of gun-powder, muſt depend much upon the forms and ſizes of its grains.

I ſhall add only one obſervation more, relative to the particular appearances which attended the experiments in which the elaſtic vapour generated in the

combuſtion of gun-powder was confined, and that is, with regard to a curious effect produced upon the inferior flat ſurface of the leathern ſtopper, where it was in contact with the generated elaſtic vapour. Upon removing the ſtopper, its lower flat ſurface appeared entirely covered with an extremely white powder, reſembling very light white aſhes, but which almoſt inſtantaneouſly changed to the moſt perfect black colour upon being expoſed to the air.

The ſudden change of colour in this ſubſtance, upon its being expoſed to the air, has led me to ſuſpect that the ſolid matter found in the barrel was not originally black, but that it became black merely in conſequence of its being expoſed to the air. The dirty grey colour it appeared to have immediately on its being drilled out of the cavity of the bore, where it had fixed itſelf, ſeems to confirm this ſuſpicion. An experiment made with a very ſtrong glaſs barrel would not only decide this queſtion, but would moſt probably render the experiment peculiarly beautiful and intereſting on other accounts; and I have no doubt but a barrel of glaſs might be made ſufficiently ſtrong to withſtand the force of the exploſion. Whether it would be able to withſtand the ſudden effects of the heat, I own I am more doubtful; but as the ſubject is ſo very intereſting, I think it would be worth while to try the experiment. Perhaps the apparatus might be ſo contrived as to ſet fire to the powder by the ſolar rays, by means of a common burning glaſs; but even if that method ſhould fail, there are others equally unexceptionable, which might certainly be employed with ſucceſs; and it is

hardly

hardly poſſible to imagine any thing more curious than an experiment of this kind would be, if it were ſucceſsful.

But to proceed to the experiments by which I endeavoured to aſcertain the force of fired gun-powder. All the parts of the apparatus being ready, it was in the autumn of the year 1792, that the firſt experiment was made.

The barrel being charged with 10 grains of powder (its contents when quite full, amounting to about 28 grains), and the end of the barrel being covered by a circular piece of oiled leather, and the flat ſide of the hemiſphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 lbs. avoirdupois, being placed upon its caſcabel in a vertical poſition, upon this hemiſphere, in order to confine by its weight the generated elaſtic fluid, the heated iron ball was applied to the end of the vent tube; and I had waited but a very few moments in anxious expectation of the event, when I had the ſatisfaction of obſerving that the experiment had ſucceeded.

The report of the exploſion was extremely feeble, and ſo little reſembling the uſual report of the exploſion of gun-powder, that the by-ſtanders could not be perſuaded that it was any thing more than the cracking of the barrel, occaſioned merely by its being heated by the red-hot ball; yet, as I had been taught by the reſult of former experiments not to expect any other report, and as I found upon putting my hand upon the barrel that it began to be ſenſibly warm, I was ſoon convinced that the powder muſt have taken fire;

and after waiting four or five minutes, upon causing the weight which rested upon the hemisphere to be raised, the confined elastic vapour rushed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choaked up by the solid substance which I have already described, and from which it was with some difficulty that it was freed, and rendered fit for another experiment.

The extreme feebleness of the report of the explosion, and the small degree of force with which the generated elastic fluid rushed out of the barrel upon removing the weight which had confined it, had inspired my assistants with no very favourable idea of the importance of these experiments. I had seen, indeed, from the beginning, by their looks, that they thought the precautions I took to confine so inconsiderable a quantity of gun-powder as the barrel could contain, perfectly ridiculous; but the result of the following experiment taught them more respect for an agent, of whose real force they had conceived so very inadequate an idea.

In this second experiment, instead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the steel hemisphere, with its oiled leather under it, was pressed down upon the end of the barrel by the same weight which was employed for that purpose in the first experiment, namely, a cannon, weighing 8081 lbs.

In order to give a more perfect idea of the result of this important experiment, it may not be amiss to describe more particularly one of the principal parts of the apparatus employed in it, I mean the barrel.

This

This barrel (which though ſimilar to it in all reſpects, was not the ſame that has already been deſcribed,) was made of the beſt hammered iron, and was of uncommon ſtrength. Its length was $2\frac{3}{4}$ inches; and though its diameter was alſo $2\frac{3}{4}$ inches, the diameter of its bore was no more than $\frac{1}{4}$ of an inch, or leſs than the diameter of a common gooſe quill. The length of its bore was 2.15 inches. Its diameter being $2\frac{3}{4}$ inches, and the diameter of its bore only $\frac{1}{4}$ of an inch, the thickneſs of the metal was $1\frac{1}{4}$ inch; or, it was 5 times as thick as the diameter of its bore.

The charge of powder was extremely ſmall, amounting to but little more than $\frac{1}{10}$ of a cubic inch; not ſo much as would be required to load a ſmall pocket piſtol, and not *one-tenth part* of the quantity frequently made uſe of for the charge of a common muſket.

I ſhould be afraid to relate the reſult of this experiment, had I not the moſt indiſputable evidence to produce in ſupport of the facts. This inconſiderable quantity of gun-powder, when it was ſet on fire by the application of the red-hot ball to the vent-tube, exploded with ſuch inconceivable force as to *burſt the barrel aſunder* in which it was confined, notwithſtanding its enormous ſtrength; and with ſuch a loud report as to alarm the whole neighbourhood.

It is impoſſible to deſcribe the ſurpriſe of thoſe who were ſpectators of this phænomenon.—They literally turned pale with affright and aſtoniſhment, and it was ſome time before they could recover themſelves.

The barrel was not only completely burſt aſunder, but the two halves of it were thrown upon the ground in different directions: one of them fell cloſe by my feet, as I was ſtanding near the machinery to obſerve more accurately the reſult of the experiment. Though I thought it poſſible that the weight might be raiſed, and that the generated elaſtic vapour would make its eſcape, yet the burſting of the barrel was totally unexpected by me. It was a new leſſon to teach me caution in theſe dangerous purſuits.

It affords me peculiar ſatisfaction in laying theſe accounts before the Public to be able to produce the moſt reſpectable teſtimony of their authenticity.

My friend, Sir CHARLES BLAGDEN, formerly Secretary of the Royal Society, viſited Munich in the ſummer of the year 1793, in his return from Italy; and though I was then abſent (travelling for the recovery of my health), yet, by my directions, he was not only ſhewn every part of the apparatus made uſe of in theſe experiments, but ſeveral experiments were actually repeated in his preſence; and he was kind enough to take with him to England one half of the barrel which was burſt in the experiment juſt mentioned, which, at my requeſt, he has depoſited in the Muſeum of the Society, and which, I flatter myſelf, will be looked upon as an unequivocal proof of my diſcoveries relative to the amazing force of the elaſtic vapour generated in the combuſtion of gun-powder.

When the amazing ſtrength of this barrel is

conſidered, and the ſmallneſs of the capacity of its bore, it appears almoſt incredible, that ſo ſmall a quantity of powder as that which was employed in the experiment could burſt it aſunder.

But without inſiſting on the teſtimony of ſeveral perſons of reſpectable character, who were eye witneſſes of the fact, and from whom Sir CHARLES BLAGDEN received a verbal account, in detail, of all the circumſtances attending the experiment, I fancy I may very ſafely reſt my reputation upon the ſilent teſtimony which this broken inſtrument will bear in my favour; much doubting whether it be in the power of art to burſt aſunder ſuch a maſs of ſolid iron, by any other means than thoſe I employed.

Before I proceed to give an account of my ſubſequent experiments upon this ſubject, I ſhall ſtop here, for a moment, to make an eſtimate,—from the known ſtrength of *iron*, and the area of the fracture of the barrel,—of the real force employed by the elaſtic vapour to burſt it.

In a courſe of experiments upon the ſtrength of various bodies, which I began many years ago, and an account of which I intend at ſome future period to lay before the Public,* I found, by taking

* Since writing the above, I have met with a misfortune which has put it out of my power to fulfil this promiſe. On my return to England from Germany, in October, 1795, after an abſence of eleven years, I was ſtopped in my poſt-chaiſe, in St. Paul's church-yard, in London, at ſix o'clock in the evening, and robbed of a trunk which was behind my carriage, containing all my private papers, and my original notes

taking the mean of the results of several experiments, that a cylinder of good tough hammered iron, the area of whose transverse section was only $\frac{3}{1000}$ of an inch, was able to sustain a weight of 119 lbs. avoirdupois, without breaking. This gives 63,466 lbs. for the weight which a cylinder of the same iron whose transverse section is one inch, would be able to sustain without being broken.

The area of the fracture of the barrel before mentioned was measured with the greatest care, and was found to measure very exactly $6\frac{1}{2}$ superficial inches. If now we suppose the iron of which this barrel was formed, to be as strong as that whose strength I determined (and I have no reason to suspect it to be of an inferior quality), in that case, the force actually employed in bursting the barrel must have been equal to the pressure of a weight of 412529 lbs. For the resistance or cohesion of one inch, is to 63466 lbs. as that of $6\frac{1}{2}$ inches to 412529 lbs.; and this force, so astonishingly great, was exerted by a body which weighed less than 26 grains Troy, and which acted in a space that hardly amounted to $\frac{1}{10}$ of a cubic inch.

To compare this force exerted by the elastic vapour generated in the combustion of gun-powder,

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notes and observations on philosophical subjects. By this cruel robbery, I have been deprived of the fruits of the labours of my whole life; and have lost all that I held most valuable. This most severe blow has left an impression on my mind, which I feel that nothing will ever be able entirely to remove. It is the more painful to me, as it has clouded my mind with suspicions that never can be cleared up.

and

and by which the barrel was burſt, to the preſſure of the atmoſphere, it is neceſſary to determine the area of a longitudinal ſection of the bore of the piece. Now the diameter of the bore being $\frac{1}{4}$ of an inch, and its length (after deducting 0,15 of an inch for the length of the leathern ſtoppers) 2 inches, the area of its longitudinal ſection turns out to have been $\frac{1}{2}$ an inch. And if now we aſſume the mean preſſure of the atmoſphere = 15lbs. avoirdupois for each ſuperficial inch, this will give $7\frac{1}{2}$ lbs for that upon a ſurface = $\frac{1}{2}$ inch, equal to the area of a longitudinal ſection of the bore of the barrel.

But we have juſt found that the force actually exerted by the elaſtic vapour in burſting the barrel, amounted to 412529 lbs.; this force was therefore 55004 times greater than the mean preſſure of the atmoſphere! For it is as $7\frac{1}{2}$ lbs. to 1 atmoſphere, ſo 412529 lbs. to 55004 atmoſpheres.

Thinking it might perhaps be more ſatisfactory to know the *real ſtrength* of the identical iron of which the barrel uſed in the before mentioned experiment was conſtructed, rather than to reſt the determination of the ſtrength of the barrel upon the deciſion of the ſtrength of iron taken from another parcel, and which very poſſibly might be of a different quality, ſince writing the above, I have taken the trouble to aſcertain the ſtrength of the iron of which the barrel was made, which was done in the following manner. Having the one half of the barrel ſtill in my poſſeſſion, I cauſed ſmall pieces, 2 inches long, and about $\frac{1}{8}$ of an inch ſquare, to be cut out of the ſolid block of metal, in the

the direction of its length, with a fine ſaw; and theſe pieces being firſt made round in their middle by filing, and then by turning in a lathe with a very ſharp inſtrument, were reduced to ſuch a ſize as was neceſſary, in order to their being pulled aſunder in my machine for meaſuring the ſtrength of bodies. In this machine the body to be pulled aſunder is held faſt by two ſtrong vices, the one faſtened to the floor, and the other ſuſpended to the ſhort arm of a Roman balance, or common ſteel-yard; and, in order that the bodies ſo ſuſpended may not be injured by the jaws of the vices, ſo as to be weakened and to vitiate the experiments, they are not made cylindrical, but they are made larger at their two ends where they are held by the vices, and from thence their diameters were gradually diminiſhed towards the middle of their lengths, where their meaſures were taken, and where they never failed to break.

As I had found by the reſults of many experiments, which I had before made upon the ſtrength of the various metals, that iron, as well as all other metals, is rendered much ſtronger by hammering, I cauſed thoſe pieces of the barrel which were prepared for theſe experiments to be ſeparated from the ſolid block of metal, and reduced to their proper ſizes, by ſawing, filing, and turning, and without ever receiving a ſingle blow of a hammer; ſo that there is every reaſon to believe that the ſtrength of the iron, as determined by the experiments, may ſafely be depended on. The reſults of the experiments were as follows:

Experi-

Experiments.	Diameter of the Cylinder at the Fracture.	Area of a tranſverſe ſection of the Cylinder at the Fracture.	Weight required to break it. lbs. avoirdupois.	Weight required to break 1 inch of this iron. lbs. avoirdupois.
	Inch.	Inch.		
1.	$\frac{50}{1000}$	$\frac{1}{509,29}$	123.18	62737.
2.	$\frac{60}{1000}$	$\frac{1}{353,68}$	182.	64366.
3.	$\frac{66}{1000}$	$\frac{1}{292,3}$	220.75	64526.
4.	$\frac{76}{1000}$	$\frac{1}{220,7}$	277.01	61063.
			Number of Experiments = 4.)	252692.
			Mean	63173.

If now we take the ſtrength of the iron of which the barrel was compoſed, as here determined by actual experiments, and compute the force required to burſt the barrel, it will be found equal to the preſſure of a weight of 410624½ lbs. inſtead of 436800 lbs. as before determined. For it is the reſiſtance or force of coheſion of 1 inch of this iron to 63173 lbs., as that of 6½ inches (the area of the fracture of the barrel) to 410624½ lbs. And this weight turned into atmoſpheres, in the manner above deſcribed, gives 54750 atmoſpheres for the meaſure of the force which muſt have been exerted by the elaſtic fluid in burſting the barrel. But this force, enormous as it may appear, muſt ſtill fall ſhort of the real initial force of the elaſtic fluid generated in the combuſtion of gun-powder, before it has began to expand; for it is more than probable that the barrel was in fact burſt before the gene-

generated elaſtic fluid had exerted all its force, or that this fluid would have been able to have burſt a barrel ſtill ſtronger than that uſed in the experiment.—But I wave theſe ſpeculations in order to haſten to more intereſting and more ſatisfactory inveſtigations.

Paſſing over in ſilence a conſiderable number of promiſcuous experiments, which, having nothing particularly remarkable in their reſults, could throw no new light upon the ſubject, I ſhall proceed immediately to give an account of a regular ſet of experiments, undertaken with a view to the diſcovery of certain determined facts, and proſecuted with unremitting perſeverance.

Theſe experiments were made by my directions, under the immediate care of Mr. REICHENBACH, commandant of the corps of artificers in the Elector's military ſervice, and of Count SPRETI, firſt lieutenant in the regiment of artillery.

Though I was prevented, by ill health, from being actually preſent at all theſe experiments, yet, being at hand, and having every day, and almoſt every hour, regular reports of the progreſs that was made in them, and of every thing extraordinary that happened, the experiments may be ſaid, with great truth, to have been made under my immediate direction; and as the two gentlemen by whom I was aſſiſted, were not only every way qualified for ſuch an undertaking, but had been preſent, and had aſſiſted me in a number of ſimilar experiments, which I had myſelf made, they had acquired all that readineſs and dexterity in the various manipulations

pulations which are ſo uſeful and neceſſary in experimental inquiries; and I think I can ſafely venture to ſay that the experiments may be depended on.

It would have afforded me great ſatisfaction to have been able to ſay that the experiments were all made by myſelf; and I had reſolved to repeat them before I made them public, particularly as there appear to have been ſome very extraordinary and quite unaccountable differences in the reſults of thoſe made in different ſeaſons of the year; but having hitherto been prevented by ill health, and by various avocations, from engaging in theſe laborious reſearches, I have thought it right not to delay any longer the publication of facts, which appear to me to be both new and intereſting, as their being known, may perhaps excite others to engage in their farther inveſtigation.

The principal objects I had in view in the following ſet of experiments were, firſt, to determine the expanſive force of the elaſtic vapour generated in the combuſtion of gun-powder, in its various ſtates of condenſation, and to aſcertain the ratio of its elaſticity to its denſity: and ſecondly, to meaſure, by one deciſive experiment, the utmoſt force of this fluid in its moſt denſe ſtate; that is to ſay, when the powder completely fills the ſpace in which it is fired, and in which the generated fluid is confined. As theſe experiments were very numerous, and as it will be more ſatisfactory to be able to ſee all their reſults at one curſory view,

I have

I have brought them into the form of a general table.

In this table, which does not ſtand in need of any particular explanation, may be ſeen the reſults of all theſe inveſtigations.

The dimenſions of the barrel made uſe of in the experiments mentioned in this table, were as follows.

Diameter of the bore at its muzzle = 0.25 of an inch.

Joint capacities of the bore, and of its vent tube, excluſive of the ſpace occupied by the leathern ſtopper, = 0.08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent tube when both were quite full, (excluſive of the ſpace occupied by the leathern ſtopper,) 25.641 German apothecary's grains, = 24½ grains Troy.

The capacities of the barrel and of its vent tube were determined by filling them with mercury, and then weighing, in the air, and in water, the quantity of mercury required to fill them; and the quantity of powder required to fill the barrel and its vent tube was determined by computation, from the known joint capacities of the barrel and its vent tube, in parts of a cubic inch, and from the known ſpecific gravity of the powder uſed in the experiments.

Thus, the contents of the barrel and its vent tube having been found to amount to 0.08974 of a cubic inch, and it having been found that 1 cubic inch of the gun-powder in queſtion, well ſhaken to-

gether

gether, weighed juſt 272.68 grains Troy, this gives 24.47 grains Troy (= 25.641 grains German apothecary's weight) for the contents of the barrel and its vent tube.

The numbers expreſſing the charges of powder in *thouſandth parts* of the joint capacities of the barrel and of its vent tube, were determined from the known quantities of powder uſed in the different experiments, expreſſed in German apothecary's grains, and the relation of theſe quantities to the quantity required to fill the barrel and its vent tube completely.

Thus, as the barrel and its vent tube were capable of containing 25.641 apothecary's grains of powder, if we ſuppoſe this quantity to be divided into 1000 equal parts, this will give 39 of thoſe parts for 1 grain; 78 parts for 2 grains; 390 for 10 grains, &c. For it is 25.641 to 1000, as 1 to 39, very nearly.

As this method of expreſſing the quantities of powder ſhows at the ſame time the relative denſity of the generated elaſtic fluid, it is the more ſatisfactory on that account: it will alſo conſiderably facilitate the computations neceſſary, in order to aſcertain the ratio of the elaſticity of this fluid to its denſity.

The elaſtic force of the fluid generated in the combuſtion of the charge of powder, is meaſured by the weight by which it was confined, or rather by that which it was juſt able to move, but which it could not raiſe ſufficiently to blow the leathern

ſtopper quite out of the mouth of the bore of the barrel.

This weight, in all the experiments, except thoſe which were made with very ſmall charges of powder, was a piece of ordnance, of greater or leſs dimenſions, or greater or leſs weight, according to the force of the charge; placed vertically upon its caſcabel, upon the ſteel hemiſphere which cloſed the end of the barrel; and the ſame piece of ordnance, by having its bore filled with a greater or ſmaller number of bullets, as the occaſion required, was made to ſerve for ſeveral experiments.

The weight employed for confining the generated elaſtic fluid, is expreſſed in the following table in *pounds avoirdupois*; but in order that a clearer and more perfect idea may be formed of the real force of this elaſtic fluid, I have added a column in which its force, anſwering to each charge of powder, is expreſſed in *atmoſpheres*.

The numbers in this column were computed in the following manner. The diameter of the bore of the barrel at its muzzle being juſt $\frac{1}{4}$ of an inch, the area of its tranſverſe ſection is 0.049088 of a ſuperficial inch; and, aſſuming the mean preſſure of the atmoſphere upon 1 ſuperficial inch equal to 15 lbs. avoirdupois, this will give 0.736331 of a pound avoirdupois, for that preſſure upon 0.049088 of a ſuperficial inch, or upon a ſurface equal to the area by which the generated elaſtic fluid acted on the weight employed to confine it; conſequently the weight expreſſed in *pounds avoirdupois*, which meaſured the force of the generated elaſtic fluid, in any given experiment, being

divided

divided by 0.73631, will ſhow how many times the preſſure exerted by the fluid was greater than the mean preſſure of the atmoſphere. Thus in the experiment, N° 6, where the weight which meaſured the elaſtic force of the generated fluid was = 504.8 lbs. avoirdupois, it is $\frac{504.8}{0.73631} = 685.6$ atmoſpheres. And ſo of the reſt.

I have ſaid that the diameter of the bore of the barrel, made uſe of in the following experiments, was juſt $\frac{1}{4}$ of an inch *at its muzzle*, and this is ſtrictly true, as I found upon meaſuring it with the greateſt care; but its diameter is not perfectly the ſame throughout its whole length, being rather narrower towards its lower end: yet the *capacity* of the barrel being known, and alſo *the diameter of the bore of its muzzle*, any ſmall inequalities of the bore in any other part can in no wiſe affect the reſults of the experiments, as will be evident to thoſe who will take the trouble to conſider the matter for a moment, with attention. I ſhould not indeed have thought it neceſſary to mention this circumſtance, had I not been afraid that ſome one who ſhould calculate the joint capacities of the bore, and of the vent-tube, from their lengths and diameters, finding their calculation not to agree with my determination of thoſe capacities, as aſcertained by filling them with mercury, might ſuſpect me of having committed an error. The *mean* diameter of the bore of barrel, as determined from its length and its capacity, turns out to be juſt 0.2281 of an inch; the diameter of the vent-tube being taken equal to 0.07 of an inch, and its length 1.715 inch.

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Experiments on the Force of fired Gun-powder.

No. of the Experiment	Time when the Experiment was made. 1793.	State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
		Thermom.	Barometer.	In Apoth. gr.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In Atmospheres	
No	h. m.	F.	Eng. In.	gr.	Parts.	lbs.		The generated elastic fluid was completely confined, the weight not being raised.
1	23d Feb. 9	31°	28.58	1	39	504.8	——	
2	9 30	—	——	2	78	——		
3	25th 9 0	37°	28.56	3	117	——	——	Ditto
4	10 15	—	——	4	156	——	——	Ditto, weight not raised.
5	10 30	—	——	5	195	——	——	Ditto, ditto.
6	11 0	—	——	6	234	——	685.6	Weight just moved.
7	3 0PM	57°	28.37	1	39	14.16		In these three experiments the weight was raised with a report as loud as that of a pistol.
8	3 15	—	——	—	—	26.5	——	
9	3 30	—	——	—	—	38.9		
10	3 45	—	——	—	—	51.3	——	Just raised, rep. much weaker.
11	4 0	—	——	—	—	57.4	77.86	Weight hardly moved.
12	26th 9 0	34°	28.1	2	78	163.5	——	Not raised.
13	9 15	—	——	—	—	124	——	Raised with a loud report.
14	9 30	—	——	—	—	130.5	——	Ditto, the report weaker.
15	9 45	—	——	—	—	133	——	Ditto, the report still weaker.
16	10 0	—	——	—	—	134.2	182.3	Weight but just moved.
17	3 0	48°	28.31	3	117	186.3	——	Raised with a loud report.
18	3 15	—	——	—	—	198.7	——	Ditto, ditto.
19	3 30	—	——	—	—	204.8	——	Ditto, report weaker.
20	3 45	—	——	—	—	208.5	——	Raised, report weaker.
21	4 0	—	——	—	—	212.24	288.2	The weight hardly moved, no report.
22	27th 3 0	50°	28.36	4	156	269.2	——	Raised with a loud report.
23	3 15	—	——	—	—	274.13	——	Ditto, ditto.
24	3 30	—	——	—	—	277.9	——	Ditto, report less loud.
25	3 45	—	——	—	—	281.57	382.4	Weight hardly moved and no report.
26	28th 9 0	34°	28.32	5	195	319.68	——	Raised, loud report.
27	9 15	—	——	—	—	351.37	——	Ditto, ditto.
28	9 30	—	——	—	—	400.9	——	Ditto, ditto.
29	10 0	—	——	—	—	475.2	——	Not raised.
30	3 0	48°	28.35	—	—	443.5	——	Not raised.
31	3 15	—	——	—	—	425.65	——	Not raised.
32	3 30	—	——	—	—	419.46	——	Not raised.

TABLE

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No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. gr.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In Atmospheres	
No		h. m.	F.	Eng. In.	gr.	Parts.	lbs.		
33	28th Feb.	3 45	48°	28.35	5	195	413.27	561.2	Weight but just moved.
34	1st March	9 0	34°	28.35	7	273	535.79	——	Raised with a loud report.
35		9 15	—	—	—	——	548.14	——	Ditto, ditto.
36		9 30	—	—	—	——	560,52	——	Ditto, ditto.
37		3 0	59°	28.34	—	——	572.9	——	Ditto, ditto.
38		3 15	—	—	—	——	585.28	——	Ditto, report weaker.
39		3 30	—	—	—	——	597.66	811.7	Weight but just moved, no report.
40		3 45	—	—	8	312	690.52	——	Raised; report very loud.
41		4 0	—	—	—	——	752.42	——	Ditto, ditto.
42		4 15	—	—	—	——	783.37	——	Ditto, ditto.
43	2d	9 0	50°	28.32	—	——	876.22	——	Not raised.
44		9 15	—	—	—	——	845.19	——	But just raised, report weak.
45		9 30	—	—	—	——	857.64	1164.8	Weight but just moved, and no report.
46		9 45	—	—	9	351	961.65	——	Raised with a loud report.
47		10 0	—	—	—	——	1209.4	——	Not raised.
48		10 30	—	—	—	——	1142.3	1551.3	Weight just moved, no report.
49		3 0	52°	28.33	10	390	1456.8	——	Not raised.
50		3 30	—	—	—		1329.9	——	Raised, loud report.
51	5th	9 0	32°	28.2	—	——	1387.5	1884.3	Weight but just moved, and no report.
52		9 15	—	—	11	429	1708.2	——	Not raised.
53		9 45	—	—	—	——	1646.2	——	Not raised.
54		10 15	—	—	—	——	1615.2	——	Raised, with a weak report.
55		10 45	—	—	—	——	1634	2219	Weight but just moved, and no report.
56	6th	9 0	36°	28.34	1	468	1943.3	——	Not raised.
57		9 30	—	—	—	——	1932.2	——	Not raised.
58		10 30	—	—	—	——	1907.4	——	Weight not raised.
59		11 0	—	—	—	——	1878.4	——	Raised with a loud report.
60		11 30	—	—	—	——	1895.1	2573.7	Weight but just moved, and no report.
61		3 0	42°	28.3	13	507	2142.7	——	Raised with a loud report.
62		3 15	—	—	—	——	2204.6	——	Ditto, ditto.

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Experiments on the Force of fired Gun-powder.

No. of the Experiment.	Time when the Experiment was made. 1793.	State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
		Thermom.	Barometer.	Apoth. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In Atmospheres	
N°	h. m.	F.	Eng. In.	gr.	Parts.	lbs.		
63	6th Mar. 3 30	42°	28.3	13	507	2266.5	——	Raised with a loud report.
64	3 45	——	——	——	——	2390.3	——	Raised, report weaker.
65	4 0	——	——	——	——	2422	3288.3	Weight just moved, no report.
66	9th. 9 0	43°	28.31	14	546	3213	——	Not raised.
67	9 30	——	——	——	——	3093	——	Not raised.
68	10 0	——	——	——	——	2968	——	Not raised.
69	10 30	——	——	——	——	2846	——	Raised with a loud report.
70	10 45	——	——	——	——	2908	——	Raised, report weaker.
71	11 0	——	——	——	——	2939	——	Ditto, report still weaker.
72	11 15	——	——	——	——	2951	4008	Weight but just moved, no report.
73	11 30	——	——	15	585	3750	——	Not raised.
74	11 45	——	——	——	——	3508	——	Not raised.
75	12 15	——	——	——	——	3477	4722.5	Weight but just moved, and no report.
76	11th 9 0	43°	28.3	16	624	4037	——	The weight was raised with a loud report.
77	9 15	——	——	——	——	4284	——	Raised, loud report.
78	9 30	——	——	——	——	4532	——	Ditto, ditto
79	4th Apr. 3 0	70°	28.2	——	——	5027	——	Ditto, ditto
80	3 15	——	——	——	——	5138	——	Raised, report weaker.
81	3 30	——	——	——	——	5262	——	Not raised.
82	3 45	——	——	——	——	5220	7090	Weight just moved, but no report.
83	5th 3 0	68°	28.3	17	663	8081	——	Not raised.
84	3 30	——	——	18	702	8081	10977	The weight was raised with a very sharp report, louder than that of a well-loaded musket.
85	4 0	——	——	——	——	8700	——	The vent tube of the barrel was burst, the explosion being attended with a very loud report.

The barrel being rendered unfit for further ſervice, by the burſting of its vent tube, an end was put to this ſet of experiments.

In order that a clear and ſatisfactory idea may be formed of the reſults of theſe experiments I have drawn the figure 11, Plate VI. in which the given denſities of the generated elaſtic fluid, or (which amounts to the ſame thing) the quantities of powder uſed for the charge, being taken on the line A B, from A towards B, the correſponding elaſticities, as found by the experiments, are repreſented by lines perpendicular to the line A B, at the points where the meaſures of the denſities end.

As the irregularities of the dotted line A C are owing, no doubt, merely to the errors committed in making the experiments, theſe irregularities being removed, by drawing the line A D in ſuch a manner as to balance the errors of the experiments, this line A D, which muſt neceſſarily be regular, will, by bare inſpection, give us a conſiderable degree of inſight into the nature of the equation which muſt be formed to expreſs the relation of the denſities to the elaſticities; the diſcovery of which was one principal object of theſe experimental enquiries.

Putting the denſity $= x$, and the elaſticity $= y$, the line A D will be the locus of the equation expreſſing the relation of x to y; and had Mr. Robins's ſuppoſition, that the elaſticity is as the denſity, been true, x would have been found to be to y in a conſtant (ſimple) ratio,—A D would have been a ſtraight line,—and A E would have been the poſition of this line, had Mr. Robins's determina-

tion of the force of fired gun-powder been accurate.

But A D is a curve, and this ſhows that the ratio of x to y is variable; and moreover it is a curve *convex towards the line* A B, on which x is taken; and this circumſtance proves that the ratio of y to x is continually increaſing.

Though theſe experiments all tend to ſhow that the ratio of y to x increaſes as x is increaſed, yet when we conſider the ſubject with attention, we ſhall, I think, find reaſon to conclude, that the exponent of that ratio can never be leſs than *unity*; and farther, that it muſt of neceſſity have *that value preciſely*, when, the denſity being taken infinitely ſmall, or $= 0$, x and y vaniſh together.

Suppoſing this to be the caſe, namely, that the exponent of the ultimate ratio of y to x is $= 1$, let the denſities, or ſucceſſive values of x be expreſſed by a ſeries of natural numbers,

0, 1, 2, 3, 4, &c. to 1000,

the laſt term $= 1000$ anſwering to the greateſt denſity; or when the powder completely fills the ſpace in which it is confined; then, by putting $z =$ the variable part of the exponent of the ratio of y to x,

To each of the ſucceſſive values of $x =$	0, 1, 2, 3, 4, &c.
The correſponding value of y will be accurately expreſſed by the equations.	0^{1+z}, 1^{1+z}, 2^{1+z}, 3^{1+z}, 4^{1+z}, &c.

For,

For, as the variable part (z), of this exponent may be taken of *any dimenſions*, it may be ſo taken at each given term of the ſeries, (or for each particular value of x), that the equation $x^{1+z} = y$, may always correſpond with the reſult of the experiments; and when this is done, the value of z, and the law of its increaſe as x increaſes, will be known; and this will ſhow the relation of x to y, or of the elaſticities of the generated fluid to their correſponding denſities, in a clear and ſatisfactory manner.

Without increaſing the length of this paper ſtill more (it being perhaps already too voluminous), by giving an account in detail of all the various computations I made, in order, from the reſults of the experiments in the foregoing Table, to aſcertain the real value of z, and the rate at which it increaſes as x is increaſed, I ſhall content myſelf with merely giving the general reſults of theſe inveſtigations, and referring for farther information to the following Table II, where the agreement of the law founded on them, with the reſults of the foregoing experiments, may be ſeen.

Having, from the reſults of the experiments in Table I. computed the different values of z, correſponding to all the different denſities, or different charges of powder, from 1 grain, or 39 *thouſandth parts*, to 18 grains, or 702 *thouſandth parts* of the capacity of the barrel, I found, that while the denſity of the elaſtic fluid $= x$, expreſſed in *thouſandth parts*, is increaſed from 0 to 1000 (or till the powder completely fills the ſpace in which it is confined), the variable part z of the exponent of x,

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$(1 + z)$ is increaſed from 0 to $\frac{4}{10}$. And though ſome of the experiments, and particularly thoſe which were made with large charges of powder, ſeemed to indicate that while x is increaſed with an equable or uniform motion, z increaſes with a motion continually accelerated; *yet*, as the reſults of by far the greateſt number of the other experiments ſhowed the velocity of the increaſe of z to be *equable*, this circumſtance, added to ſome other reaſons drawn from the nature of the ſubject, have induced me to aſſume the ratio of the increaſe of z to the increaſe of x, as conſtant.

But if, while x increaſes with an equable velocity from 0 to 1000, z is increaſed with an *equable velocity* from 0 to $\frac{4}{10}$, then it is every where z to x, as $\frac{4}{10}$ to 1000; or $1000\,z = \frac{4}{10}$, and conſequently $z = \frac{4x}{10000}$; and when x is $= 1$, it is $z = \frac{4}{10000} = 0.0004$; and when x is greater or leſs than 1, it is $z = 0.0004x$; and z being expunged, the general equation expreſſing the relation of x to y becomes $x^{1+0.0004x} = y$; and this is the equation which was made uſe of in computing the values of y, expreſſed in the following Table.

In order that the elaſticities might be expreſſed in atmoſpheres, the values of y, as determined by this equation, were multiplied by 1.841.

If it be required to expreſs the elaſticity in *pounds avoirdupois*, then the value of y, as determined by the foregoing equation, being multiplied by 27.615, will ſhow how many pounds avoirdupois, preſſing upon a ſuperficial inch, will be equal to the preſſure exerted by the elaſtic fluid in the caſe in queſtion.

TABLE II.

General Results of the Experiments in Table I. on the Force of fired Gun-powder.

The Charge of Powder.		Value of the Exponent. $1+0.0004x$.	Computed Elasticity of the generated Fluid, or value of y, according to the Theorem, $x^{1+0.0004x}=y$.		Actual Elasticity, as shown by the Experiments.	Difference of the computed and the actual Elasticities.
In Grains.	In equal Parts.		In equal Parts.	In Atmospheres.	In Atmospheres.	In Atmospheres.
1	39	1.0156	41.294	76.822	77.86	+ 1.838
2	78	1.0312	89.357	164.506	182.30	+ 17.794
3	117	1.0468	146.210	269.173	228.2	− 40.973
4	156	1.0624	213.784	393.577	382.4	− 11.177
5	195	1.0780	294.209	541.640	561.2	+ 19.560
6	234	1.0936	389.919	717.841	685.6	− 32.241
7	273	1.1092	503.723	927.353	811.7	−115.653
8	312	1.1248	638.889	1176.19	1164.8	− 12.390
9	351	1.1404	799.223	1471.37	1551.3	+ 79.930
10	390	1.1560	989.169	1821.06	1884.3	+ 63.240
11	429	1.1716	1213.91	2234.81	2219.	− 15.810
12	468	1.1872	1479.50	2723.77	2573.7	−150.07
13	507	1.2028	1793.	3300.91	3283.3	− 17.61
14	546	1.2184	2162.69	3980.52	4008.	+ 27.48
15	585	1.2340	2598.18	4783.26	4722.5	− 60.76
16	624	1.2496	3110.73	5726.83	7090.	+1363.17
17	663	1.2652	3713.46	6836.46		
18	702	1.2808	4421.69	8140.34	10977.	+2836.66
19	741	1.2964	5253.3	9671.33		
20	780	1.3120	6229.14	11467.8		
25.641	1000	1.4000	15848.9	29177.9		

The agreement of the elasticities computed from the theorem $x^{1+0.0004x}=y$, with the actual elasticities as they were measured in the experiments, may be seen in the foregoing Table; but this agreement may

be

be ſeen in a much more ſtriking manner by a bare inſpection of the figure 11, Plate VI.; for the line A D, in this figure, having been drawn from the computed elaſticities, its general coincidence with the line A C ſhows how nearly the computed and the actual elaſticities approach each other. And when the irregularities of the line A C (which, as had already been obſerved, muſt be attributed to the unavoidable errors of the experiments), are corrected, theſe two curves will be found to coincide with much preciſion, throughout a conſiderable part of the range of the experiments; but towards the end of the ſet of experiments, when the charges of powder were conſiderably increaſed, the elaſticities ſeem to have increaſed faſter than, according to the aſſumed law, they ought to have done.

From this circumſtance, and from the immenſe force the charge muſt have exerted in the experiment, when the barrel was burſt, I was led to ſuſpect that the elaſtic force of the fluid generated in the combuſtion of gun-powder, when its denſity is great, is ſtill much greater than theſe experiments ſeem to indicate; and a farther inveſtigation of the ſubject ſerved to confirm me in this opinion.

It has been ſhown that the force exerted by the charge in the experiment in which the barrel was burſt could not have been leſs than the preſſure of 54,752 atmoſpheres; but the greateſt force of the generated elaſtic fluid, when, the powder filling the ſpace in which it is confined, its denſity is $= \frac{7}{1000}$,

1000, on computing its elaſticity by the theorem $x^{1+0.0004x}=y$, turns out to be only equal to 29,178 atmoſpheres.

In this computation the mean of the results of all the experiments in the foregoing ſet is taken as a ſtandard to aſcertain the value, expreſſed in atmoſpheres, of y, and it is $y \times 1.841 = 29,178$.

But if, inſtead of taking the mean of the whole ſet of experiments as a ſtandard, we ſelect that experiment in which the force exerted by the powder appears to have been the greateſt, yet, in this caſe, even the initial force of fired gun-powder, computed by the above rule, would be much too ſmall.

In the experiment N° 84, when the charge conſiſted of 18 grains of powder, and the denſity, or value of x was 702, a weight equal to the preſſure of 10,977 atmoſpheres was raiſed. Here the value of y $(=x^{1+0.0004x})$ is found to be $(702^{1.2808})$, = 4421.7; and to expreſs this value of y in atmoſpheres, and at the ſame time to accommodate it to the actual reſult of the experiment, it muſt be multiplied by 2.4826; for it is 4421.7 (the value of y expreſſed in equal parts) to 10,977 (its value in atmoſpheres, as ſhown by the experiment), as 1 to 2.4826, and conſequently $4421.7 \times 2.4826 = 10,977$.

If now the value of y be computed on the ſame principles, when x is put $= 1000$, it will turn out to be $y = 1000^{1+0.4} = 15,849$; and this number expreſſed in atmoſpheres, by multiplying it by 2.4826, gives the value of $y = 39,346$ atmoſpheres.

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This however falls ſtill far ſhort of 54,752 atmoſpheres, the force the powder was actually found to exert when the charge filled the ſpace in which it was confined. But in the 84th experiment, when 18 grains of powder were uſed, as the weight (8081 lbs. avoirdupois) was raiſed with *a very loud report*, it is more than probable that the force of the generated elaſtic fluid was in fact conſiderably greater than that at which it was eſtimated, namely, greater than the preſſure of 10,977 atmoſpheres.

But, without waſting time in fruitleſs endeavours to reconcile anomalous experiments, which, probably, never can be made to agree, I ſhall haſten to give an account of another ſet of experiments; the reſults of which, it muſt be confeſſed, were ſtill more various, extraordinary, and inexplicable.

The machinery having been repaired and put in order, the experiments were recommenced in July, 1793, the weather at that time being very hot.

The principal part of the apparatus, *the barrel*, had undergone a trifling alteration: upon refitting and cleaning it, the diameter of its bore at the muzzle was found to be a little increaſed, ſo that a weight equal to 8081 lbs. avoirdupois, inſtead of being equal to 10977 atmoſpheres (as was the caſe in the former experiments), was now juſt equal to the preſſure of 9431 atmoſpheres.

Though I was not at Munich when this laſt ſet of experiments was made, they however were undertaken at my requeſt, and under my direction, and I have no reaſon to doubt of their having been executed with all poſſible care. They were all

made

made by the ſame perſons who were employed in making the firſt ſet; and as theſe experimenters may be ſuppoſed to have grown expert in practice, and as they could not poſſibly have had any intereſt in deceiving me, I cannot ſuſpect the accuracy of their reports.

TABLE III.

Experiments on the Force of fired Gun-powder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmoſphere.		The Charge of Powder.		Weight employed to confine the elaſtic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth.grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In Atmoſpheres	
N°		h m.	F.	Eng.In.	grs.	Parts.	lbs.		
86	1ſt July	4 0	88°	28.37	17	663	8081	9431	The weight was raiſed with an aſtoniſhing loud report.
87		4 30	—	—	—	—	—	—	In theſe three experiments the weight was raiſed with a very loud report.
88		4 45	—	—	16	624	—	—	
89		5 0	—	—	15	585	—	—	
90		5 30	—	—	12	468	—	—	Weight not raiſed.
91		6 0	—	—	13	507	—	9431	Weight but juſt raiſed, report very weak.
92	2d	9 0	71°	28.38	—	—	—	—	Raiſed, loud report.
93		9 30	—	—	12	468	—	—	Raiſed, feeble report.
94		10 0	—	—	—	—	—	9431	Raiſed, report very feeble.
95		10 30	80°	—	$11\frac{7}{8}$	—	—	—	*Juſt moved*, no report.
96	3d	10 0	70°	28.55	12	468	—	—	Not raiſed.
97		10 30	—	—	13	507	—	—	Not raiſed.
98		11 0	75°	—	14	546	—	9431	Juſt raiſed, feeble report.
99	4th	9 0	70°	28.56	14	546	—	—	Not raiſed.
100		9 30	—	—	—	—	—	—	Not raiſed.
101		10 0	72°	—	15	585	—	—	The weight was raiſed, the report not very loud.
102		10 30	—	—	$15\frac{1}{2}$	—	—	—	Nearly as above.
103	8th	9 0	74°	28.42	—	—	—	—	Raiſed, and with an uncommonly loud report.
104		9 30	—	—	13	507	—	—	Raiſed, report very loud.
105		10 45	85°	—	12	468	—	9431	But juſt raiſed, the report very feeble.
106	17th	9 0	75°	28.4	—	—	—	—	Nearly as above.
107		9 45	—	—	—	—	—	—	Ditto.
108		10 30	—	—	$11\frac{1}{4}$	—	—	—	*Juſt moved*, no report.
109		11 0	—	—	—	—	—	—	The ſame as above.

It

It appears from the foregoing Table, that, in the afternoon of the 1ſt of July, the weight (which was a heavy braſs cannon, a 24 pounder, weighing 8081 lbs. avoirdupois), was not raiſed by 12 grains of powder, but that 13 grains raiſed it with an audible though weak report.—That, the next morning, July 2d, at 10 o'clock, it was raiſed twice by charges of 12 grains.—That, in the morning of the 3d of July, it was not raiſed by 12 grains nor by 13 grains; but that 14 grains juſt raiſed it.—That, in the afternoon of the ſame day, two experiments were made with 14 grains of powder, in neither of which the weight was raiſed; but that in another experiment, in which 15 grains of powder were uſed, it was raiſed with a moderate report.—That, in the morning of the 8th of July, in two experiments, one with $15\frac{1}{2}$ grains, and the other with 13 grains of powder, the weight was raiſed with a *loud report*; and in an experiment with 12 grains, it was raiſed with a *feeble report*. And laſtly, that in two ſucceſſive experiments, made in the morning of the 17th of July, the weight was raiſed by charges of 12 grains.

Hence it appears, that under circumſtances the moſt favourable to the developement of the force of gun-powder, a charge (= 12 grains) filling $\frac{468}{1000}$ of the cavity in which it is confined, on being fired, exerts a force againſt the ſides of the containing veſſel, equal to the preſſure of 9431 atmoſpheres; which preſſure amounts to 141465 lbs. avoirdupois on each ſuperficial inch.

Mr. ROBINS makes the initial, or greateſt force

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of the fluid generated in the combuſtion of gun-powder, (namely, when the charge completely fills the ſpace in which it is confined), to be only equal to the preſſure of 1000 atmoſpheres. It appears, however, from the reſult of theſe experiments, that, even admitting the elaſticities to be as the denſities, as Mr. ROBINS ſuppoſes them to be, the initial force of this generated elaſtic fluid muſt be at leaſt twenty times greater than Mr. ROBINS determined it;—for $\frac{468}{1000}$, the denſity of the elaſtic fluid in the experiments in queſtion, is to 1, its denſity when the powder quite fills the ſpace in which it is confined, as 9431 atmoſpheres, the meaſure of its elaſtic force in the experiments in queſtion, to 20108 atmoſpheres; which, according to Mr. ROBINS's theory reſpecting the ratio of the elaſticities to the denſities, would be the meaſure of its initial force.

But all my experiments tend uniformly to prove, that the elaſticities increaſe *faſter* than in the ſimple ratio of the correſponding denſities; conſequently the initial force of the generated elaſtic fluid *muſt neceſſarily* be greater than the preſſure of 20108 atmoſpheres.

In one of my experiments, which I have often had occaſion to mention, the force actually exerted by the fluid muſt have been at leaſt equal to the preſſure of 54752 atmoſpheres. The other experiments ought, no doubt, to ſhew, at leaſt, that it is *poſſible* that ſuch an enormous force may have been exerted by the charge made uſe of; and this, I think, they actually indicate.

In the firſt ſet of experiments, which were made when the weather was cold, though the refults of them uniformly ſhewed the force of the powder to be much leſs than it appeared to be in all the ſubſequent experiments, made with greater charges and in warm weather, yet they all ſhew that the ratio of the elaſticity of the generated fluid to its denſity is very different from that which Mr ROBINS's theory ſuppoſes; and that this ratio increaſes as the denſity of the fluid is increaſed.

Suppoſing (what on many accounts ſeems to be extremely probable) that this ratio increaſes uniformly, or with an equable celerity, while the denſity is uniformly augmented; and ſuppoſing farther, that the velocity and limit of its increaſe have been rightly determined from the reſult of the ſet of experiments, Table I. which were made with that view; then, from the reſult of the experiments of which we have juſt been giving an account, (in which 12 grains of powder exerted a force equal to 9431 atmoſpheres), taking theſe experiments as a ſtandard, we can, with the help of the theorem ($x^{1}+^{0.0004x}=y$) deduced from the former ſet of experiments, compute the initial force of fired gun-powder, thus:

The denſity of the elaſtic fluid, when 12 grains of powder are uſed for the charge, being = 468, it is $468^{1.1872}=y=1479.5$; and in order that this value of y may correſpond with the reſult of the experiment, and be expreſſed in atmoſpheres, it muſt be multiplied by a certain co-efficient, which will be found by dividing the value of y expreſſed

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in atmoſpheres, as ſhewn by the experiment, by the number here found indicating its value, as determined by computation.

It is therefore $\frac{9431}{1479.5} = 6.3744$ for the value of this co-efficient; and this multiplied into the number 1479.5 gives 9431 for the value of y in atmoſpheres.

Again, the denſity being ſuppoſed = 1000 (or, that the charge of powder completely fills the cavity in which it is confined), in that caſe it will be $1000^{1+0.4} = y = 15849$; and this number being turned into atmoſpheres by being multiplied by the co-efficient above found (= 6.3744), gives 101021 atmoſpheres for the meaſure of the *initial force* of the elaſtic fluid generated in the combuſtion of gun-powder.

Enormous as this force appears, I do not think it over-rated; for nothing much ſhort of ſuch an inconceivable force can, in my opinion, ever explain in a ſatisfactory manner, the burſting of the barrel ſo often mentioned; and to this we may add, that, as in 7 different experiments, all made with charges of 12 grains of powder, there were no leſs than 5 in which the weight was *raiſed with a report*; and as the ſame weight was *moved* in 3 different experiments in which the charge conſiſted of leſs than 12 grains, there does not appear to be any reaſon whatever for doubt with regard to the principal fact on which the above computation is founded.

There is an objection, however, that may be made to theſe deciſions reſpecting the force of gun-pow-

der, which, on the firſt view, appears to be of conſiderable importance; but on a more careful examination it will be found to have no weight.

If the force of fired gun-powder is ſo very great, how does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withſtand ſo enormous a force, are not always burſt when they are uſed? I might anſwer this queſtion by another, by aſking how it happened that the barrel uſed in my experiments, and which was more than 10 times ſtronger in proportion to the ſize of its bore than ever a piece of ordnance was formed, could be burſt by the force of gun-powder, if its force is not in fact much greater than it has ever been ſuppoſed to be? But it is not neceſſary to have recourſe to ſuch a ſhift to get out of this difficulty: there is nothing more to do than to ſhow, which may eaſily be done, that the combuſtion of gun-powder is leſs rapid than it has hitherto been ſuppoſed to be, and the objection in queſtion falls to the ground.

Mr. Robins's theory ſuppoſes that all the powder of which a charge conſiſts is not only ſet on fire, but that it is actually *conſumed* and "*converted into an elaſtic fluid before the bullet is ſenſibly moved from its place.*" I have already, in the former part of this paper, offered ſeveral reaſons which appeared to me to prove that, though the *inflammation* of gun-powder is very rapid, yet the progreſs of the combuſtion is by no means ſo *inſtantaneous* as has been imagined. I ſhall now give

give an account of ſome experiments which put that matter out of all doubt.

It is a fact well known, that on the diſcharge of fire-arms of all kinds, cannon and mortars, as well as muſkets, there is always a conſiderable quantity of unconſumed grains of gun-powder blown out of them; and, what is very remarkable, and as it leads directly to a diſcovery of the cauſe of this effect is highly deſerving of conſideration, theſe unconſumed grains are not merely blown out of the *muzzles* of fire-arms; they come out alſo by their vents or touch-holes, *where the fire enters to inflame the charge;* as many perſons who have had the misfortune to ſtand with their faces near the touch-hole of a muſket, when it has been diſcharged, have found to their coſt.

Now it appears to me to be extremely improbable if not abſolutely impoſſible, that a grain of gun-powder, actually in the chamber of the piece, and completely ſurrounded by flame, ſhould, by the action of that very flame, be blown out of it, without being at the ſame time ſet on fire. But if theſe grains of powder are *actually on fire* when they come out of the piece, and are afterwards found at a diſtance from it *unconſumed*, this is, in my opinion, a moſt deciſive proof, not only that the combuſtion of gun-powder is by no means ſo rapid as it has generally been thought to be, but alſo (what will doubtleſs appear quite incredible), that if a grain of gun-powder, actually on fire, and burning with the utmoſt violence over the whole extent of its ſurface, be projected with *a very great velocity* into

a cold atmoſphere, the fire will be extinguiſhed, and the remains of the grain will fall to the ground, unchanged, and as inflammable as before.

This extraordinary fact was aſcertained beyond all poſſibility of doubt by the following experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gun-powder, all of the ſame maſs, but formed into grains of very different ſizes, ſome as ſmall as the grains of the fineſt Battel powder, and the largeſt of then nearly as big as large peaſe, I placed a number of vertical ſcreens of very thin paper, one behind another, at the diſtance of 12 inches from each other; and loading a common muſket repeatedly with this powder, ſometimes without, and ſometimes with a wad, I fired it againſt the foremoſt ſcreen, and obſerved the quantity and effects of the unconſumed grains of powder which impinged againſt it.

The ſcreens were ſo contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The diſtance from the muzzle of the gun to the firſt ſcreen was not always the ſame; in ſome of the experiments it was only 8 feet, in others it was 10, and in ſome 12 feet.

The charge of powder was varied in a great number of different ways, but the moſt intereſting experiments were made with one ſingle large grain of powder, propelled ſometimes by ſmaller and

ſometimes

ſometimes by larger charges of very fine-grained powder.

Theſe large grains never failed to reach the ſcreen; and though they ſometimes appeared to have been broken into ſeveral pieces, by the force of the exploſion, yet they frequently reached the firſt ſcreen entire; and ſometimes paſſed through all the ſcreens (five in number), without being broken.

When they were propelled by large charges, and conſequently with great velocity, they were ſeldom on fire when they arrived at the firſt ſcreen, which was evident, not only from their not ſetting fire to the paper (which they ſometimes did), but alſo from their being found ſticking in a ſoft board, againſt which they ſtruck, after having paſſed through all the five ſcreens; or leaving viſible marks of their having impinged againſt it, and being broken to pieces and diſperſed by the blow. Theſe pieces were often found lying on the ground; and from their forms and dimenſions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminiſhed by the combuſtion, before the fire was put out, on the globe being projected into the cold atmoſphere. The holes made in the ſcreen by the little globe in its paſſage through them, ſeemed alſo to indicate that its diameter had been diminiſhed.

That theſe globes, or large grains of powder, were always ſet on fire by the combuſtion of the charge can hardly be doubted. This certainly

happened in many of the experiments, for they arrived at the ſcreens on fire, and ſet fire to the paper; and in the experiments in which they were projected with ſmall velocities, they were often ſeen to paſs through the air on fire; and when this was the caſe no veſtige of them was to be found.

They ſometimes paſſed, on fire, through ſeveral of the foremoſt ſcreens without ſetting them on fire, and ſet fire to one or more of the hindmoſt, and then went on and impinged againſt the board, which was placed at the diſtance of 12 inches behind the laſt ſcreen.

It is hardly neceſſary for me to obſerve, that all theſe experiments prove that the combuſtion of gun-powder is very far from being ſo inſtantaneous as has generally been imagined. I will juſt mention one experiment more, in which this was ſhown in a manner ſtill more ſtriking, and not leſs concluſive. A ſmall piece of red-hot iron being dropped down into the chamber of a common horſe piſtol, and the piſtol being elevated to an angle of about 45 degrees, upon dropping down into its barrel one of the ſmall globes of powder (of the ſize of a pea), it took fire, and was projected into the atmoſphere by the elaſtic fluid generated in its own combuſtion, leaving a very beautiful train of light behind it, and diſappearing all at once, like a falling ſtar.

This amuſing experiment was repeated very often, and with globes of different ſizes. When very ſmall ones were uſed ſingly, they were commonly conſumed entirely before they came out of the barrel

barrel of the piſtol; but when ſeveral of them were uſed together, ſome, if not all of them, were commonly projected into the atmoſphere on fire.

I ſhall conclude this paper by ſome obſervations on the practical uſes and improvements that may probably be derived from theſe diſcoveries, reſpecting the great expanſive force of the fluid generated in the combuſtion of gun-powder.

As the *ſlowneſs* of the combuſtion of gun-powder is undoubtedly the cauſe which has prevented its enormous and almoſt incredible force from being diſcovered, ſo it is evident, that the readieſt way to increaſe its effects is to contrive matters ſo as to accelerate its inflammation and combuſtion. This may be done in various ways, but the moſt ſimple and moſt effectual manner of doing it would, in my opinion, be to ſet fire to the charge of powder by ſhooting (through a ſmall opening) the flame of a ſmaller charge into the midſt of it.

I contrived an inſtrument on this principle for firing cannon, ſeveral years ago, and it was found on repeated trials to be uſeful,—convenient in practice,—and not liable to accidents. It likewiſe ſuperſedes the neceſſity of uſing priming,—vent-tubes,—port-fires,—and matches; and on that account I imagined it might be of uſe in the Britiſh navy. Whether it has been found to be ſo or not I have not heard.

Another infallible method of increaſing very conſiderably the effect of gun-powder in fire-arms of all ſorts and dimenſions, would be to cauſe the bullet to fit the bore exactly, or without windage,

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in that part of the bore, at leaſt, where the bullet reſts on the charge: for when the bullet does not completely cloſe the opening of the chamber, not only much of the elaſtic fluid generated in the firſt moment of the combuſtion of the charge eſcapes by the ſides of the bullet, but, what is of ſtill greater importance, a conſiderable part of the unconſumed powder is blown out of the chamber along with it, in a ſtate of actual combuſtion, and getting before the bullet, continues to burn on as it paſſes through the whole length of the bore, by which the motion of the bullet is much impeded.

The loſs of force which ariſes from this cauſe is, in ſome caſes, almoſt incredible; and it is by no means difficult to contrive matters ſo as to render it very apparent, and alſo to prevent it.

If a common horſe piſtol be fired with a looſe ball, and ſo ſmall a charge of powder that the ball ſhall not be able to penetrate a deal board ſo deep as to ſtick in it, when fired againſt it from the diſtance of ſix feet; the ſame ball, diſcharged from the ſame piſtol, with the ſame charge of powder, may be made to paſs quite through one deal board, and bury itſelf in a ſecond place behind it, *merely by preventing the loſs of force which ariſes from what is called windage;* as I have found more than once by actual experiment.

I have in my poſſeſſion, a muſket, from which, with a common muſket charge of powder, I fire two bullets at once with the ſame velocity that a ſingle bullet is diſcharged from a muſket on the common conſtruction, with the ſame quantity of

powder.

powder. And, what renders the experiment ſtill more ſtriking,—the diameter of the bore of my muſket is exactly the ſame as that of a common muſket, except only in that part of it where it joins the chamber, in which part it is juſt ſo much contracted that the bullet which is next to the powder may ſtick faſt in it. I ought to add, that though the bullets are of the common ſize, and are conſequently conſiderably leſs in diameter than the bore, means are uſed which *effectually* prevent the loſs of force by windage; and to this laſt circumſtance it is doubtleſs owing, in a great meaſure, that the charge appears to exert ſo great a force in propelling the bullets.

That the conical form of the lower part of the bore, where it unites with the chamber, has a conſiderable ſhare in producing this extraordinary effect, is however very certain, as I have found by experiments made with a view merely to aſcertain that fact.

I will finiſh this paper by a computation, which will ſhow that the force of the elaſtic fluid generated in the combuſtion of gun-powder, enormous as it is, may be ſatisfactorily accounted for upon the ſuppoſition that its force depends *ſolely* on the elaſticity of watery vapour, or ſteam.

It has been ſhown by a variety of experiments made in England, and in other countries, and lately by a well conducted ſet of experiments made in France by M. DE BETANCOUR, and publiſhed in Paris under the auſpices of the Royal Academy of Sciences, in the year 1790, that the elaſticity

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of ſteam is doubled by every addition of temperature equal to 30 degrees of FAHRENHEIT's thermometer.

Suppoſing now a cavity of any dimenſions (equal in capacity to 1 cubic inch, for inſtance), to be filled with gun-powder, and that on the combuſtion of the powder, and in conſequence of it, this ſpace is filled with ſteam (and I ſhall preſently ſhow that the water, exiſting in the powder *as water*, is abundantly ſufficient for generating this ſteam); if we know the heat communicated to this ſteam in the combuſtion of powder, we can compute the elaſticity it requires by being ſo heated.

Now it is certain that the heat generated in the combuſtion of gun-powder cannot poſſibly be leſs than that of red-hot iron. It is probably much greater, but we will ſuppoſe it to be only equal to 1000 degrees of FAHRENHEIT's ſcale, or ſomething leſs than iron viſibly red-hot in day-light. This is about as much hotter than boiling linſeed oil, as boiling linſeed oil is hotter than boiling water.

As the elaſtic force of ſteam is juſt equal to the mean preſſure of the atmoſphere when its temperature is equal to that of boiling water, or to 212° of FAHRENHEIT's thermometer, and as its elaſticity is doubled by every addition of temperature equal to 30 degrees of the ſame ſcale, with the heat of $212° + 30° = 242°$ its elaſticity will be equal to the preſſure of 2 atmoſpheres; at the temperature of $242° + 30° = 272°$ it will equal 4 atmoſpheres;

at 272° + 30° = 302° it will equal 8 atmoſpheres;
at 302° + 30° = 332° — 16 —
at 332° + 30° = 362° — 32 —
at 362° + 30° = 392° — 64 —
at 392° + 30° = 422° — 128 —
at 422° + 30° = 452° — 256 —
at 452° + 30° = 482° — 512 —
at 482° + 30° = 512° — 1024 —
at 512° + 30° = 542° — 2048 —
at 542° + 30° = 572° — 4096 —
at 572° + 30° = 602°, (or 2 degrees above the heat of boiling linſeed oil,) its elaſticity will be equal to the preſſure of 8192 atmoſpheres, or above *eight times* greater than the utmoſt force of the fluid generated in the combuſtion of gun-powder, according to Mr. ROBINS's computation. But the heat generated in the combuſtion of gun-powder, is much greater than that of 602° of FAHRENHEIT's thermometer, conſequently the elaſticity of the ſteam generated from the water contained in the powder muſt of neceſſity be much greater than the preſſure of 8192 atmoſpheres.

Following up our computations on the principles aſſumed, we ſhall find that,

at the temperature of 602° + 30° = 632°	the elaſticity will be equal to the preſſure of 16,384 atmoſpheres;
at 632° + 30° = 662°	32,768 ——
at 662° + 30° = 692°	65,536 ——

and at 692° + 30° = 722°, the elaſticity will be equal to the preſſure of 131,072 atmoſpheres, which

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is 130 times greater than the elaſtic force aſſigned by Mr. ROBINS to the fluid generated in the combuſtion of gun-powder; and about *one ſixth* part greater than my experiments indicated it to be.

But even here the heat is ſtill much below that which is moſt undoubtedly generated in the combuſtion of gun-powder. The temperature which is indicated by 722° of FAHRENHEIT's ſcale, (which is only 122 degrees higher than that of boiling quickſilver, or boiling linſeed oil,) falls ſhort of the heat of iron which is viſibly red-hot in daylight by 355 degrees: but the flame of gun-powder has been found to melt braſs, when this metal, in very ſmall particles, has been mixed with the powder; and it is well known that to melt braſs a heat is required equal to that of 3807 degrees of FAHRENHEIT's ſcale; 2730 degrees above the heat of red-hot iron, or 3085 degrees higher than the temperature which gives to ſteam an elaſticity equal to the preſſure of 131072 atmoſpheres.

That the elaſticity of ſteam would actually be increaſed by heat in the ratio here aſſumed, can hardly be doubted. It has abſolutely been found to increaſe in this ratio in all the changes of temperature between the point of boiling water (I may even ſay of freezing water) and that of 280° of FAHRENHEIT's ſcale; and there does not appear to be any reaſon why the ſame law ſhould not hold in higher temperatures.

A doubt might poſſibly ariſe with reſpect to the exiſtence of a ſufficient quantity of water in gun-powder, to fill the ſpace in which the powder is

fired, with ſteam, at the moment of the exploſion; but this doubt may eaſily be removed.

The beſt gun-powder, ſuch as was uſed in my experiments, is compoſed of 70 parts (in weight) of nitre), 18 parts of ſulphur, and 16 parts of charcoal; hence 100 parts of this powder contain $67\frac{3}{10}$ parts of nitre, $17\frac{3}{10}$ parts of ſulphur, and of charcoal $15\frac{4}{10}$ parts.

Mr. KIRWAN has ſhown, that in 100 parts of nitre there are 7 parts of water of cryſtallization; conſequently, in 100 parts of gun-powder, as it contains $67\frac{3}{10}$ parts of nitre, there muſt be $4\frac{711}{1000}$ parts of water.

Now as 1 cubic inch of gun-powder, when the powder is well ſhaken together, weighs exactly as much as 1 cubic inch of water at the temperature of 55° F. namely 253.175 grains Troy, a cubic inch of gun-powder in its drieſt ſtate muſt contain at leaſt $10\frac{927}{1000}$ grains of water; for it is 100 to 4.711, as 253.175 to 10.927. But beſides the water of chryſtallization which exiſts in the nitre, there is always a conſiderable quantity of water in gun-powder, in that ſtate in which it makes bodies *damp* or *moiſt*. Charcoal expoſed to the air has been found to abſorb nearly $\frac{1}{8}$ of its weight of water; and by experiments I have made on gun-powder, by aſcertaining its loſs of weight on being much dried, and its acquiring this loſt weight again on being expoſed to the air, I have reaſon to think that the power of the charcoal, which enters into the compoſition of gun-powder, to abſorb water remains unimpaired, and that it actually retains as much

much water in that ſtate, as it would retain were it not mixed with the nitre and the ſulphur.

As there are $15\frac{4}{10}$ parts of charcoal in 100 parts of gun-powder, in 1 cubic inch of gun-powder (= 253.175 grains Troy,) there muſt be 38.989 grains of charcoal; and if we ſuppoſe $\frac{1}{8}$ of the apparent weight of this charcoal to be water, this will give 4.873 grains in weight for the water which exiſts in the form of *moiſture* in 1 cubic inch of gun-powder.

That this eſtimation is not too high is evident from the following experiment. 1160 grains Troy of apparently dry gun-powder, taken from the middle of a caſk, on being expoſed 15 minutes in dry air, heated to the temperature of about 200°, was found to have loſt 11 grains of its weight. This ſhews that each cubic inch of this gun-powder actually gave out $2\frac{4}{10}$ grains of water on being expoſed to this heat; and there is no doubt but that at the end of the experiment it ſtill retained much more water than it had parted with.

If now we compute the quantity of water which would be ſufficient, when reduced to ſteam under the mean preſſure of the atmoſphere, to fill a ſpace equal in capacity to 1 cubic inch, we ſhall find that, either that contained in the nitre which enters into the compoſition of 1 cubic inch of gun-powder as *water of cryſtallization*, or even that ſmall quantity which exiſts in the powder in the ſtate of *moiſture*, will be much more than ſufficient for that purpoſe.

Though the denſity of ſteam has not been determined with that degree of preciſion that could be wiſhed, yet it is quite certain that it cannot be leſs than 2000 times rarer than water, when both are at the temperature of 212°. Some have ſuppoſed it to be more than 10,000 times rarer than water, and experiments have been made which ſeem to render this opinion not improbable; but we will take its denſity at the higheſt eſtimation, and ſuppoſe it to be only 2000 times rarer than water. As 1 cubic inch of water weighs 253.175 grains, the water contained in 1 cubic inch of ſteam at the temperature of 212° will be $\frac{1}{2000}$ part of 253.175 grains, or 0.12659 of a grain.

But we have ſeen that 1 cubic inch of gun-powder contains 10.927 grains of water of cryſtallization, and 4.873 grains in a ſtate of moiſture. Conſequently the quantity of water of cryſtallization in gun-powder is 86 times greater, and the quantity which exiſts in it in a ſtate of *moiſture* is 38 times greater, than that which would be required to form a quantity of ſteam ſufficient to fill completely the ſpace occupied by the powder.

Hence we may venture to conclude, that the quantity of water actually exiſting in gun-powder, is much more than ſufficient to generate all the ſteam that would be neceſſary to account for the force diſplayed in the combuſtion of gun-powder (ſuppoſing that force to depend ſolely on the action of ſteam), even though no water ſhould be generated in the combuſtion of the gun-powder. It is even

very probable that there is more of it than is wanted, and that the force of gun-powder would be ſtill greater, could the quantity of water it contains be diminiſhed.

From this computation it would appear, that the difficulty is not to account for the force actually exerted by fired gun-powder, but to explain the reaſon why it does not exert a much greater force. But I ſhall leave theſe inveſtigations to thoſe who have more leiſure than I now have to proſecute them.

SUPPLEMENTARY OBSERVATIONS.

Although there is no reaſon to doubt of the accuracy of M. BETANCOUR's experiments, yet, there is one important point which ſtill remains to be aſcertained, before the hypotheſis I have here endeavoured to eſtabliſh on the reſults of thoſe experiments, reſpecting the force exerted by ſteam in combuſtion of gun-powder, can be admitted.

The ſteam, the elaſtic force of which was meaſured in M. BETANCOUR's experiments, *remained conſtantly in contact with water, in a liquid ſtate.*— How far did the preſence of this water, and the progreſſive change of a part of it to ſteam, as the heat was gradually increaſed, and the addition to the *denſity* of the ſteam which reſulted therefrom, contribute to the increaſe of the elaſticity of the ſteam which was obſerved?

This is a very important queſtion, and the ſolu-

tion

tion of it muſt neceſſarily decide the fate of our hypotheſis: and after mature conſideration, I am myſelf inclined to think that I have been precipitate in aſcribing too much to the agency of ſteam in the force exerted by fired gun-powder. I was led into this error, if it be one, on finding the explanation of the cauſe of the force of gun-powder given by Mr. ROBINS to be quite inadequate to its effects, as ſhewn in my experiments. But I did not at firſt advert to the degree in which one of the ſuppoſitions made by Mr. ROBINS, (namely, that reſpecting the heat of the generated elaſtic fluid) is gratuitous; nor did I then perceive how very probable it is that he has greatly under-rated it.

The ſuppoſition of Mr. ROBINS reſpecting the heat was ſuch as enabled him to reconcile the reſults of *his experiments* with *his theory*; and, as later diſcoveries have ſhewn his theory to be unfounded, ſome of his aſſumed principles muſt of neceſſity have been erroneous.

The moſt unexceptionable of the ſuppoſitions of Mr. ROBINS relative to the ſubject under conſideration, is that reſpecting the quantity of air, or *permanently elaſtic fluid*, that is generated from gun-powder in its combuſtion. It cannot indeed, with propriety be called a ſuppoſition, for it was the reſult of a well-contrived experiment.

According to Mr. ROBINS, when any given quantity of gun-powder is fired, the quantity of permanently elaſtic fluid, or fluids generated from it, in its combuſtion, is ſuch, that, when cooled down to the mean temperature of the atmoſphere, it would,—

under the mean preſſure of the atmoſphere—fill a ſpace 250 greater than that which the unfired powder occupied. Conſequently, if this fluid were compreſſed into a ſpace no greater than that occupied by unfired powder, it would, in its endeavours to expand itſelf, on being ſo compreſſed, exert a force 250 times greater than the mean preſſure of the atmoſphere. Or, (to uſe the language employed in the foregoing paper)—its force would be equal to 250 atmoſpheres.

This is very far indeed from 100,000 atmoſpheres,—the expanſive force we have aſſigned to fired gun-powder,—but let us ſee how far *the heat* generated in the combuſtion of gun-powder may be ſuppoſed to increaſe the expanſive force of the elaſtic fluids which are generated in that proceſs.

From the experiments of the late General Roy, it has been proved, that the expanſive force of common atmoſpherical air is doubled with an increaſe of heat indicated by 437 degrees of Fahrenheit's ſcale; and I was lately informed by that excellent chemiſt, and natural philoſopher, M. Bertholet, that he has found by repeated experiments, that the expanſions with heat of all the gazes, or different kinds of permanently elaſtic fluids, are preciſely the ſame, whatever may be the difference of their ſpecific gravities, or of their chemical or other properties.

Suppoſing now that the permanently elaſtic fluids generated in the combuſtion of gun-powder follow the ſame law in their expanſions with heat, we can eaſily determine, by computation, how much

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the expanſive force of thoſe fluids will be augmented by any given augmentation of heat.

If the temperature of air of the atmoſphere be 60 F. when the expanſive force of the permanently elaſtic fluid generated in the combuſtion of gunpowder,—*being at that temperature*—is equal to 250 atmoſpheres; if temperature of that fluid be raiſed 437 degrees, or if it become 497° of Fahrenheit's ſcale, (60° + 437° = 497°.) There can be no doubt whatever but its elaſtic force will be doubled; or that it will become 500 times greater than the mean preſſure of the atmoſphere.

If its temperature be raiſed 437 degrees higher, or if be heated to 934° F.—its elaſticity will be again doubled, and will become = 1000 atmoſpheres, which is the initial force of the elaſtic fluid generated in the combuſtion of fired gun-powder, according to Mr. ROBINS.

But there are many ſtrong reaſons for ſuppoſing that the heat generated in the combuſtion of gunpowder is vaſtly higher than that indicated by 937° of Fahrenheit's ſcale.

If the heat be only that indicated by 3996° F. (which is many degrees below that at which either copper, ſilver, or gold melts) that heat will be ſufficient to double the expanſive force laſt found (= 1000 atmoſpheres) *ſeven times*, which will make it equal to the preſſure of *one hundred and twenty eight thouſand atmoſpheres*;—a degree of elaſtic force conſiderably more than ſufficient to account for the reſults of all the experiments mentioned in this paper.

A SHORT ACCOUNT

OF SOME

EXPERIMENTS MADE WITH CANNON,

AND ALSO OF SOME

ATTEMPTS TO IMPROVE FIELD ARTILLERY.

DURING my residence in Bavaria, I had an opportunity of verifying, upon a large scale, the method proposed in my first Paper on Gun-powder, for measuring the velocities of bullets by the recoil of the gun; and I had also opportunities of making several other interesting experiments connected with that subject. In the spring of the year 1791, a large building was erected, for the express purpose of pursuing these investigations, in the neighbourhood of Munich, on the ground destined for the exercise of the artillery, where a most complete apparatus was put up for measuring the velocities of cannon bullets, by the recoil of the gun; and also by the pendulum, at the same time; and with this apparatus, a great number of interesting experiments were made, under my direction, and most of them in my presence. I should long ago have laid an account of them before the public, had I not been induced to postpone their publication by a desire that they might make a part of a work on artillery, yet unfinished, but which I hope, at some future period, to be able to send to the press.

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In this work I ſhall give a detailed account, illuſtrated by accurate plans (which are now ready for the engraver), of all the changes in the conſtruction of the Bavarian Artillery, which were introduced in my attempts to improve it, during the time the military affairs of that country were under my direction: In the mean time I have thought it adviſeable to give, in this place, a ſhort account of ſuch of my experiments as are moſt intimately connected with the ſubjects of the two preceding papers; and this I ſhall now do in as few words as poſſible.

Pieces of braſs ordnance, of three different calibres, viz. 3 pounders,—6 pounders,—and 12 pounders,—having been ſuſpended, in an horizontal poſition, by long pendulous rods, or bars of iron, in the manner deſcribed in one of the foregoing papers, theſe guns were fired in this ſituation, with different charges of powder; with, and alſo without bullets; and ſometimes with two, and with three, fit bullets at the ſame time; and the velocity of the gun in its recoil having been determined, in each experiment, by the length (meaſured by means of a ribband) of the chord of the aſcending arc of its firſt vibration, from that velocity, and the known weights of the gun, and of the bullet, the velocity of the bullet was computed.

The gun having been pointed againſt the centre of a very large heavy pendulum, conſtructed of ſtrong timbers, well faſtened together with iron, the bullets lodged in that pendulum; and their velocities were determined, according to Mr. Robins's method, by the arcs of the vibration of the pen-

dulum; and theſe two methods of aſcertaining the velocities of the bullets were found to agree with great accuracy.

This pendulum, although it was made very ſtrong, was, however, ſoon deſtroyed by the bullets; but it was not rendered uſeleſs till after a ſufficient number of experiments had been made with it to eſtabliſh, beyond all doubt, the accuracy of the propoſed method of determining the velocities of cannon bullets by the recoil of the gun. As ſoon as this was done, the pendulum, being no longer wanted, was removed, and the bullets were fired into a mound of earth, which had been thrown up to receive them.

The general reſults of this courſe of experiments were as follow:—With the ſame charge of powder, the velocities of *two*, and *three*, fit bullets, diſcharged at once, from the cannon, were found to be greater than the velocity of a *ſingle bullet*, impelled by the given charge, in a proportion conſiderably higher than that determined by Mr. Robins.

Means were employed, which prevented entirely the eſcape, by windage, of the elaſtic fluid generated from the powder in its combuſtion, and this added very conſiderably to the apparent force of the charge.

The force of the charge was always ſenſibly increaſed when the gun was diſcharged by firing a piſtol (conſtructed for that uſe) into the vent, inſtead of uſing a priming, and a common match, for firing off the gun.

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As I have entered upon this ſubject; and as it is poſſible that I may never find leiſure to finiſh the work on Artillery, which, for many years, I have had in hand, I cannot reſiſt the inclination I feel to avail myſelf of this opportunity to ſubmit to the public,—but more eſpecially to profeſſional men—ſome of the principal reſults of my experiments and meditations in the proſecution of my inquiries, relative to the improvement of artillery.

Thoſe who are engaged in theſe reſearches may, perhaps, derive ſome advantage from the hints, however curſory they may be, of a perſon who has long been in a habit of obſerving; and who has had many opportunities of making intereſting experiments.

When I was called to take the direction of the military affairs of the late Elector Palatine, Duke of Bavaria, the army was deſtitute of a well-organized train of field artillery; and there was no Cannon Foundry in Bavaria that was in a condition to be uſed. The arſenal at Munich was filled with cannon, but by far the greater part of them were perfectly uſeleſs, being very ancient, and too heavy and unwieldy to be moved. There was a very good Cannon Foundry at Manheim, the capital of the Elector's dominions upon the Rhine, but the diſtance between Munich and Manheim is ſo great, that it would have coſt more to have ſent the Bavarian guns to Manheim, to be refounded, and to have brought them back by land carriage, than was required to defray the expence of eſtabliſhing a new manufactory for the conſtruction of artillery in Bavaria.

A Foundry

A Foundry was accordingly eſtabliſhed at Munich, and neither pains nor expence were ſpared to make it as perfect as poſſible; and a moſt excellent machine was erected for boring cannon; with workſhops adjoining to it, for the conſtruction of gun carriages, and ammunition waggons.

With theſe advantages, and with a ſet of good workmen at my command, in all the different branches of mechanics that are concerned in the conſtruction of artillery, it will readily be believed, by thoſe who know how much my attention had been employed on that ſubject, that I did not neglect to avail myſelf of ſo favourable an opportunity, to make trial of ſome of the plans for the improvement of artillery, which had been recommended by profeſſional men of eminence; or that had been the reſult of my own experiments and meditations.

The following *Memoir*, that accompanied a model of a Bavarian field piece, with its ammunition waggon, which, with the permiſſion of the late Elector, I had the honour to preſent to the United States of North America (my native country), will give the reader a general idea of ſome of the alterations that were introduced, in the conſtruction of that ſpecies of ordnance, in my attempts to improve Field Artillery.*

The model above mentioned was upon a large ſcale, being *one quarter of the full ſize* in length, diameter, &c. and it was finiſhed, and mounted in

* This gun, with all the details of its carriage, drawn to a ſcale, are repreſented in the (annexed) Plates VII. VIII. and IX.

the

the moſt complete manner, and fitted for actual ſervice or experiment.—I brought it with me to England, from Bavaria, in the autumn of the year 1798, and before it was ſhipped for America, I had the honour to ſhow it to his Royal Highneſs the Duke of York, commander in chief of his Majeſty's forces in Great Britain; and to leave it ſeveral months for the inſpection of the officers of the Royal Artillery.

" MEMOIR, *in which ſome account is given of the New Bavarian Field Pieces, lately conſtructed at Munich, under the direction of Lieutenant General, Count* Rumford.

" In contriving theſe guns (which are 6 pounders, 18 calibres in length), the following objects were had in view.

" 1. To conſtruct them in ſuch a manner as to render them capable of being uſed occaſionally as flying, or horſe artillery.

" Proviſion is made for carrying occaſionally all the men belonging to the gun, upon the limber, and upon the ammunition waggon, where ſafe and commodious ſeats are provided for them.

" To ſave time in getting the gun into action, the two men who ride upon the limber, jump down from their ſeat the inſtant the gun arrives upon the ground where it is to be poſted, and unlimber, while the other men who ride on the ammunition waggon are coming up.

" The two long pieces of aſh timber, which form the

the principal part of the body of the ammunition waggon, (uniting the fore wheels to the hind) act as ſprings, by their elaſticity: and, in order that their action may be as free as poſſible, they ſhould not be ſhod with iron, nor ſhould they be made too bulky.

" The long cheſt in which the greater part of the ammunition is carried, and upon which five men occaſionally ride, is ſo ſlung and confined by ſtrong ſide braces, that if the ammunition waggon ſhould be overturned, the men who are upon this cheſt—upon which they are ſeated aſtride—would be in no danger of its falling upon them.

" Although proviſion is made for carrying the men belonging to theſe guns in caſes of neceſſity, and tranſporting them with celerity from one place to another, yet it is not meant that they ſhould be allowed to ride at all times; but merely when the guns are uſed as flying artillery.

" 2. One principal object had in view was to render the gun carriage as ſtrong and durable as poſſible, without encreaſing its weight.

" A bare inſpection of this carriage, and a compariſon of it with the carriages of field pieces on the common conſtruction, will ſhow the various means that have been uſed to attain this important end; and that theſe means have been effectual, has been abundantly proved by the uncommon ſtrength which thoſe carriages evinced when they were ſubmitted to the moſt ſevere trials. Several of theſe ſix pounders were fired repeatedly with 3 lbs.

of the beſt powder, and three fit bullets, without receiving the ſmalleſt injury; while other 6 pounders of the ſame weight, mounted according to the common method, ſeldom failed to break and diſable their carriages, when expoſed to this trial, although the common carriage was 100 lbs. heavier than the new Bavarian carriage;—namely, 80 lbs. in iron, and 20 lbs. in wood.

" The flaſks of the carriages of field pieces are commonly much weakened by being made crooked; and alſo by the number of holes that are bored in them; but the new Bavarian carriage is free from both theſe defects.—The Bavarian carriage is moreover much preſerved by the collars of thick ſole leather, which ſurround and cover the trunnions of the gun, and the pivots of the elevating machine—for this ſoft and elaſtic ſubſtance, being interpoſed between the gun and the carriage, ſerves to deaden the blow of the gun againſt its carriage in the recoil.

" 3. Several new contrivances were introduced with a view to expedite the management of the gun in ſervice; and to prevent accidents and miſtakes in the uſe of the handſpikes, ram-rods, &c.

" The handſpikes (of which there are two, in order that one may remain if the other is ſhot away), are attached to the gun carriage, and, conſequently, cannot be miſplaced, nor loſt, through careleſſneſs, nor in the hurry of action. There are no buckles to unfaſten, nor cords to looſen and untie,

untie, in preparing the gun for action.—The ramrod is faſtened to the carriage in a ſafe and ſimple manner, and may be detached from it in an inſtant, when wanted; and the tompion of the gun, and the ſtopper that cloſes the vent, may be removed, or put into their places, with the greateſt expedition.

"Upon a comparative trial of one of theſe new-invented guns, with a field piece on the common conſtruction, (which trial was made in the preſence of the late Elector Palatine, reigning Duke of Bavaria), it was found, that, when both guns arrived on the ground at the ſame moment, the new gun commonly unlimbered and fired from four to ſix rounds before the old gun could be got ready to be fired once.

"4. In the mounting of this new gun, care was taken to provide for the pointing and elevating of it, in the moſt expeditious manner, and for the confining of it at any given elevation.

"5. To make proviſion for elevating it, to any given number of degrees or minutes, above the object againſt which it is pointed, without the aſſiſtance of any quadrant—plumb-line—or other inſtrument.

"The elevating machine, belonging to this gun, will be found to anſwer perfectly for all theſe purpoſes. The thread of the elevating ſcrew may, in all caſes, be ſo choſen, that one turn of the ſcrew

ſhall

ſhall elevate the gun a certain number of minutes—as 60 for inſtance—when the gun, being previouſly pointed directly at the object, may be elevated to any required number of degrees or minutes above it, merely by keeping an account of the number of revolutions, and parts of revolutions, of the elevating ſcrew that anſwer, to the given elevation.

6. " In order to facilitate the pointing of the gun, when the wheels of the gun-carriage happen to be placed upon uneven ground, there are two lines of ſight drawn upon the gun (one on each ſide of it), which are both perfectly parallel with the axis of its bore.

" The utility of theſe lines muſt be evident to thoſe who know how difficult it is to point a gun at a given object when one of the wheels of its carriage is higher than the other; and how much, under this circumſtance (of uneven ground), a gun is neceſſarily thrown out of its true direction, by pointing it by means of fixed ſights, or notches, ſituated on the upper part of the gun.

" By means of the lines of ſight, *parallel to the bore*, which are marked on theſe guns, the pointing of the gun may at any time be examined and corrected; for whenever the gun is properly pointed, either of the lines of ſight will carry the eye either againſt the object (namely, in very ſmall diſtances), or perpendicularly over it.

" 7. As it frequently happens that the wheels of gun-

gun-carriages are wounded and disabled in action, in order that a speedy remedy may be applied in those cases, in the Bavarian artillery, the wheels of the gun-carriages of the field pieces are of the same form and dimensions, precisely, as the wheels of their limbers, and the hinder wheels of their ammunition waggons, in order that these last may be taken occasionally to replace the former.

" 8. In the construction of the new Bavarian artillery, all useless reinforced rings upon the gun have been omitted, and pains have been taken to make its form as simple as possible.

(Signed) RUMFORD."

BROMPTON,
26th August, 1799.

But my attempts to improve artillery were not confined to the *form* of the gun, and its carriage;—the change that might be made, with advantage, in the *material* employed in constructing cannon for the land service, was likewise a subject of experiment which engaged my attention.

Happening to be present many years before, when the late Admiral DARBY, in a large company of Admirals and Captains of the navy, expressed his *great satisfaction* at having *at length* succeeded in his attempts *to get rid of the brass guns*, which, for many years after iron ordnance had been generally sub-

ſubſtituted for braſs, in the navy, the BRITANNIA, (the Admiral's ſhip) had continued to carry, on her lower gun-deck; the converſation to which this incident gave riſe, concerning the relative merit of iron and braſs guns, made a laſting impreſſion on my mind; and, as ſoon as it was in my power, I did not neglect to make ſuch experiments as I conceived would be ſufficient to determine a queſtion that appeared to me to be of very great magnitude indeed.

I had conceived an idea that IRON ordnance might, with much advantage, be ſubſtituted inſtead of BRASS, for field artillery, and in general for all kinds of artillery.

I made ſome experiments with a view to the aſcertaining of that fact, during the time I ſerved with my regiment in the American war, but theſe were upon too confined a ſcale to be deciſive: as far, however, as they went, they tended to confirm the favourable opinion I had conceived of the uſefulneſs of iron guns in the land ſervice. The Hon. Admiral DIGBY having been ſo kind, at my ſolicitation, as to lend me two *twelve pounder* caronnades, I mounted them as howitzers, and found them to be very uſeful guns, eſpecially when they were fired with a *grape*, compoſed of 6, or of 9 *pound balls*: but at Munich theſe experiments were made on a much more extenſive ſcale.

It had been aſſerted by thoſe who did not approve of the introduction of iron ordnance for the land ſervice, that, although iron guns of a very

large calibre, ſuch as are uſed on ſhipboard, are found to be ſufficiently ſtrong, yet, ſmaller guns, ſuch as would be proper for field artillery, would be very liable to burſt, if they were not made much heavier than braſs guns of the ſame bore; which would render them unwieldy, and unfit for thoſe rapid movements which are often ſo deciſive on a day of action.

Were this objection founded, it would undoubtedly be deciſive againſt the general introduction of iron guns; but from all the inquiries I could make I thought there was reaſon to conclude, that *want of ſtrength*, which was certainly very apparent in iron guns of inferior dimenſions, aroſe from accidental circumſtances; and that an effectual remedy might eaſily be found for that imperfection.

Until iron guns were caſt *ſolid*, or without a core, no hopes could reaſonably be entertained of being able to caſt ſmall guns of that metal ſo ſound, and to render their texture ſo uniform throughout, as to enable them to reſiſt the force of their cuſtomary charges, without reinforcing them with an unuſual quantity of metal: and even after iron guns of all calibres were caſt ſolid, yet, as the ſtrength, or toughneſs, of caſt iron depends much on the *ſlowneſs with which it cools in the mould*; and as ſmall maſſes cool more rapidly than larger ones, it is evident that guns of a ſmall ſize would be much more likely to be wanting in ſtrength, than larger cannon of a ſimilar form: but I never could ſee any reaſon for ſuppoſing that a ſmall

piece

piece of iron ordnance would be wanting in ſtrength, if means were employed for preſſing the metal with a conſiderable weight while it remains in the mould in a fluid ſtate; and for prolonging the time of its cooling.

When cannon are caſt of gun-metal, or braſs, the ſolid cylinder, or rather cone, which, when bored, conſtitutes the gun, is caſt at leaſt 2 feet longer than it is intended that the gun ſhould be. The additional piece of metal beyond the end of the muzzle, which is cut off before the gun is bored (and thrown away, or rather preſerved and melted again), ſerves to compreſs the fluid maſs in the lower part of the mould, and to force upwards thoſe bubbles of air that are frequently carried down into the mould with the deſcending current of melted metal; and which, if they were not expelled, would form what are called *honeycombs* in the metal; and would render the gun unſound, and unſerviceable.

The mould is always placed in a vertical poſition, with that end upwards which is to be the muzzle of the gun, in order that the ſoundeſt part of the metal,—namely, that which, having ſuſtained the greateſt preſſure while the metal remained in a fluid ſtate, is moſt effectually freed of air-bubbles—may be in the neighbourhood of the chamber of the piece, where ſtrength and ſoundneſs are moſt wanted.

The utility of theſe precautions being evident, they were not neglected in caſting the iron guns that were conſtructed for my experiments; and

means were uſed for prolonging the time of the cooling of the metal in the mould.

The guns caſt, were 1 pounders,—3 pounders, —6 pounders,—12 pounders,—and one 18 pounder;—and to make the compariſon with braſs artillery the more ſtriking and ſatisfactory, they were all—except the 1 pounders and the 18 pounder,—made of the ſame lengths, and of the ſame weight, as braſs guns of the ſame calibres.

Theſe iron guns were all *caſt ſolid*, and were bored in an horizontal poſition; and while the gun was boring, the ſwelling of its muzzle, and the firſt reinforce ring (at the breech,) were neatly turned. The forms of all theſe guns were ſimple, and not inelegant. Their trunnions were placed in ſuch a manner, that a line paſſing through their axes meets the axis of the bore of the gun, and cuts it at right angles. The trunnions were *turned*, and their forms and dimenſions made perfectly true, by means of a particular machine, contrived for that purpoſe.

To protect the trunnions and the gun-carriage from the violence of the blow, which the carriage receives from the gun in its recoil, the trunnions were covered with thick ſole leather, greaſed with tallow; and the trunnion plate was made to fit the trunnion—when thus covered with leather—as accurately as poſſible. The elevating ſcrew was ſo attached to the caſcable that the gun was completely confined, and prevented from kicking up, which probably contributed not a little to the protection of the carriage.

When

When theſe iron guns were mounted,—and not before—* they were *proved;* and they all ſuſtained, without the ſmalleſt injury, the moſt ſevere proof that ever had been given to braſs guns of the ſame weights, lengths, and calibres. They were then *fired, quick,* at the ſhorteſt poſſible intervals, with half the weight of their bullets in powder, and three fit bullets, one upon the other, but *not one of them burſt in theſe ſevere trials;* nor were any of their carriages injured. One of the 6 pounders, 16 calibres in length, and weighing 720 lbs. was then taken from its carriage, and being laid on the ground, was twice fired, once with 6 lbs. of powder and two bullets; and once with the ſame charge of powder and three bullets; but theſe attempts to burſt it were fruitleſs.

The reſult of theſe experiments having removed all the doubts that were entertained reſpecting the ſtrength of theſe guns; their accuracy in ſhooting was now tried. They were repeatedly fired, both with round, and with canniſter-ſhot againſt a mark; placed at different diſtances; and they were unanimouſly declared by all preſent at theſe trials, to be quite equal, for accuracy, to the beſt braſs ordnance.

Several of them were afterwards uſed in actual

* As it is not common to mount guns before they are proved, it is right that the reader ſhould know why this uſual and neceſſary precaution was neglected in this inſtance. It was done to ſurpriſe and confound thoſe who were diſpoſed to criticiſe, and prepared to oppoſe. It was, no doubt, a bold meaſure; but bold meaſures are ſometimes the moſt prudent. The 18 pounder was not ſubmitted to theſe ſevere trials.

ſervice, and were found to be as complete, and as uſeful guns as any in the ſervice.

The 1 pounders,—which were on a peculiar conſtruction—were much uſed in the defence of Manheim; and they became at laſt ſuch favourites with the corps of artillery, that the men on duty actually made intereſt with their officers to be ſtationed at them. Theſe ſmall guns were about 4 feet long in the bore; and although their calibre was that of an *one pound* iron bullet, they weighed as much as one of the 3 *pounders*, and were commonly fired, in ſervice, with *three* bullets at a time, which they carried with ſurpriſing accuracy.

When the object at which they were pointed was at a great diſtance, ſingle leaden bullets were uſed, which, in order that they might fit the bore with greater preciſion, were wrapped up in thin leather, greaſed with tallow, or ſoaked in oil.

The carriage of this gun was extremely ſimple, being a ſingle piece of elm-timber, faſtened to an iron axle. The wheels were of the ſame height and ſtrength as thoſe uſed for the carriages of 3 pounders. From this deſcription it will be evident that this little gun bears a near reſemblance to the *Ammuſette*, invented by the late Lieutenant General Deſaguillers, and conſtructed at Woolwich. The fact is, that they were copied, with a few trifling alterations, from that piece of ordnance, being made after a drawing of it which was given me by the Lieutenant General, three and twenty years ago. He, no doubt, took the idea of this gun from the reveries of Marſhal Saxe.

The

The iron 18 pounder conſtructed at Munich, —which was intended merely as an experiment,— was very ſhort, being only 10 calibres in length, and it was mounted in a very ſingular manner. It was intended for covering troops retreating before an advancing enemy, and is ſo contrived that it can be fired, without ſtopping; or while it is *in full march*. It has indeed often been fired, and very quick too, while the horſes which drew it were in full gallop. The carriage, which is upon four wheels, ſerves at the ſame time as an ammunition waggon; and alſo for carrying the men who ſerve the gun. Theſe however are only *three* in number, and more are not wanted.

After diſcharging the piece, upon pulling a ſtrap, the gun, of itſelf, falls into a vertical poſition, with its muzzle upwards. The cartridge is then put into it, and by its weight falls into its place; the ball, grape, or canniſter, with which the gun is loaded, falling into the conical opening of the chamber, there ſticks faſt, and is firmly fixed and confined in its place. By means of another ſtrap, or rope, the breech of the gun is raiſed, with the ſtrength of one man, and the piece is brought again into an horizontal ſituation (or to any given elevation); where, by means of a rack (in the form of the limb of a quadrant) and a catch, it is confined, and ready to be again diſcharged. Upon drawing back this catch, after the piece has been fired, the gun falls again into a vertical poſition.

The carriage is ſo conſtructed that the gun may, with great facility, be pointed ſeveral degrees, either

to the right or to the left of the line of the direction of the march, without altering the direction in which the horſes are going on.

The gun requires no priming, being fired by means of a piſtol, conſtructed for that purpoſe; the flame of which is impelled with ſuch violence through the vent of the gun, that it never fails to pierce the thick woollen bag, which contains the powder, and to ſet fire to the charge.

This piece carries canniſter-ſhot with great effect, but the charge, which to me appeared to be moſt formidable, and beſt calculated to intimidate an enemy, was a grape, conſiſting of 9 *two pound* iron bullets; for as theſe balls are ſufficiently large to rebound from the ground, or *recocheter*, ſeveral times, eſpecially when the gun is not much elevated, they bound on to a great diſtance; and as they exhibit all the appearances of cannon bullets to the ſpectators who ſee them arrive among them, and produce nearly the ſame effects as much larger bullets, where they take place againſt either cavalry or infantry, it is very likely, I think, that two or three of theſe guns, well plied with this grape, would make an advancing column of very brave troops heſitate, even though they ſhould come on fluſhed with victory.

I had contrived a gun, on theſe principles, that could be fired *advancing*, as well as *retreating*, in full march; but as I am not now writing a treatiſe on artillery, it would be improper for me to enlarge farther on the ſubject.

I cannot finiſh this Paper without juſt obſerving,

ſerving, with reſpect to iron guns, that if any country could ſafely venture to ſubſtitute iron ordnance inſtead of braſs, for field artillery, it would be Great Britain; for the manufacture of caſt-iron is now carried to ſuch perfection in this iſland, that it is made, at pleaſure, of almoſt any degree of hardneſs, or ſoftneſs; and if the ſaving of copper can *any where* be an object of public importance, it muſt be here, where ſo much of that metal is uſed in covering ſhips.

For my own part, I do not heſitate to ſay, that I think iron guns better than braſs guns, in every reſpect; when the metal is as good as it may be eaſily made; and they are certainly much more durable, and coſt incomparably leſs. They are likewiſe more eaſily deſtroyed, and rendered uſeleſs (by knocking off their trunnions), when it becomes neceſſary to abandon them.

I well know that theſe opinions will not meet with the general approbation of thoſe who, no doubt, ought to be conſidered as the proper and only competent judges in matters of this kind; yet I may be permitted to ſay, that my opinions have not been lightly taken up, nor haſtily formed. I ſubmit them to profeſſional men, with that deference which is due from an individual to ſo numerous and ſo reſpectable a corps of gentlemen.

EXPERIMENTS

ON THE

PRODUCTION OF AIR, FROM WATER,

EXPOSED WITH VARIOUS SUBSTANCES TO THE ACTION OF LIGHT.

[Read before the Royal Society, Feb. 15, 1787.]

VARIOUS opinions having been entertained with reſpect to the origin of the air, produced by expoſing healthy vegetables in water to the action of the ſun's rays, according to the method of Dr. INGEN-HOUSZ; and not being myſelf thoroughly ſatisfied with any of the theories propoſed for explaining the phænomena, I made the following experiments, with a view to throwing ſome new light upon that ſubject.

Having found by accident that raw ſilk poſſeſſes a power of attracting and ſeparating air from water in great abundance, when expoſed in it to the action of light, it occurred to me to examine the properties of this air, and to conſider more attentively the circumſtances attending its production, thinking that this might poſſibly lead to ſome further diſcoveries, relative to the production of the air yielded by water, under other circumſtances: and though my ſucceſs in theſe inquiries has not been equal to my

wiſhes, yet, as in the courſe of my reſearches, I have diſcovered ſome facts which I take to be new, and as I have confirmed others already known, by a variety of new experiments, I flatter myſelf that an account of my labours upon this ſubject will not be thought altogether unintereſting.

Before I enter upon the detail of my experiments it will be neceſſary to premiſe, that I ſhall in general confine myſelf merely to the facts as they preſent themſelves, without applying them to the confirmation or refutation of the theories of others, and without entering into any ſpeculative inquiries relative to their remote cauſes; and in deſcribing the different appearances I ſhall make uſe of the moſt familiar terms. Thus, in ſpeaking of the air produced upon expoſing raw ſilk in water to the action of light, I ſhall ſometimes mention it as being yielded by the ſilk; and I ſhall ſometimes ſpeak of the air furniſhed by expoſing water, which has previouſly turned green, in the ſun's rays, as being immediately produced by the water, though it is probable, that the *green matter* acts a very important part in the production of this air, in the one caſe, and perhaps in the other. But how it acts is not well aſcertained; and I had in general much rather confine myſelf to a ſimple, and even an unlearned, deſcription of facts, than, by endeavouring to give more preciſe definitions, at firſt, to involve myſelf in all the difficulties which would attend an attempt to account for phænomena, whoſe cauſes are but very imperfectly known.

Expe-

Experiment, N° 1.

My firſt object was to collect a ſufficient quantity of the air, ſeparated from water by ſilk, to determine its goodneſs by the teſt of nitrous air; and to this end, having filled with clear ſpring-water, a globe of thin, white, and very tranſparent glaſs, $4\frac{1}{2}$ inches in diameter, with a cylindrical neck $\frac{3}{4}$ of an inch in diameter, and about 12 inches long, I introduced into it 30 grains of raw ſilk, which had been previouſly waſhed in water, in order to free it of air; and, inverting the globe under water, and placing its neck in a glaſs jar, containing a quantity of the ſame water with which the globe was filled, I expoſed it in my window to the action of the ſun's rays, and prepared myſelf to examine the progreſs of the generation or production of the air.

The globe had not been expoſed ten minutes to the action of the ſun's rays, when I diſcovered an infinite number of exceedingly ſmall air-bubbles, which began to make their appearance upon the ſurface of the ſilk; and theſe bubbles continuing to increaſe in number, and in ſize, at the end of about two hours, the ſilk, appearing to be entirely covered with them, roſe to the upper part of the globe.

Theſe bubbles going on to increaſe in ſize, and running into each other, at length began to detach themſelves from the ſilk, and to form a collection of air in the upper part of the globe; but the meaſure

meaſure of my eudiometer being rather large, it was not till after the globe had been expoſed in the ſun near four days, that a ſufficient quantity of air was collected to make the experiment with nitrous air, in order to aſcertain its goodneſs by that teſt.

Having at length collected a ſufficient quantity of this air for that purpoſe, I carefully removed it from the globe, and mixing with 1 meaſure of it, 3 meaſures of nitrous air, they were reduced to 1,24 meaſures; which ſhews, that it was actually *dephlogiſticated air* *, and of a conſiderable degree of purity.

Common air, tried at the ſame time, 1 meaſure of it with 1 meaſure of nitrous air were reduced to 1,08 meaſure.

Having again expoſed the globe, with the ſame water and ſilk, in my window, where the ſun ſhone the greateſt part of the day, at the end of three days I had collected $3\frac{3}{4}$ cubic inches of air, which, proved with nitrous air, gave $1a+3n=1,18$; that is to ſay, 1 meaſure of this air, *added to* 3 meaſures of nitrous air, were reduced to 1,18 meaſure.

A wax taper, which had been juſt blown out, a ſmall part only of the wick remaining *red-hot*, upon being plunged into a phial filled with this air, immediately took fire, and burnt with a very bright and enlarged flame.

* It muſt be remembered that this was written in the year 1786, at which period this elaſtic fluid was generally denominated *dephlogiſticated air*.

The

The water in the globe appeared to have loſt ſomething of its tranſparency, and had changed its colour to a very faint greeniſh caſt, having at the ſame time acquired the odour or fragrance proper to raw ſilk.

This experiment I repeated ſeveral times with freſh water (retaining the ſame ſilk) and always with nearly the ſame reſult; with this difference, however, that when the ſun ſhone very bright, the quantity of air produced was not only greater, but its quality likewiſe was much ſuperior to that yielded when the ſun's rays were more feeble, or when they were frequently intercepted by flying clouds. The air, however, was always not only much better than common air, but better than the air in general produced by the freſh leaves of plants expoſed in water to the ſun's rays, in the experiments of Dr. INGEN-HOUSZ; and under the circumſtances the moſt favourable, it was ſo good that 1 meaſure of it required 4 meaſures of nitrous air to ſaturate it, and 3,65 meaſures of the two airs were deſtroyed; or, proved with nitrous air, it gave $1a+4n=1,35$, which, I believe, is better than any air that has yet been produced in the experiments with vegetables.

The method I have here adopted of uſing algebraic characters in noting the reſult of the experiments made to determine the goodneſs of air, though not ſtrictly mathematical, is very convenient; and for that reaſon, I ſhall continue to make uſe of it. a repreſents the air which is proved; n nitrous air; and the numbers which are joined to theſe letters

ſhew

ſhew the quantities, or the number of meaſures, of the different airs made uſe of in the experiment. The other number, which ſtands alone, or without any letter attached to it, on the other ſide of the equation, ſhews the volume, or the number of meaſures, and parts of a meaſure, to which the two airs are reduced after they are mixed. I ſhall ſometimes add a fourth number, ſhewing the quantities of the two airs deſtroyed, as this more immediately ſhews the goodneſs of the air which is proved.

Thus, in the experiment laſt mentioned, 1 meaſure of the air proved, mixed with 4 meaſures of nitrous air, were reduced to 1,35 meaſure, conſequently 3,65 meaſures of the two airs were deſtroyed; for it is $1 + 4 = 5 - 1{,}35 = 3{,}65$, and the reſult of this trial I ſhould write thus, $1a + 4n = 1{,}35$, or $3{,}65$.

Or, for ſtill greater convenience in practice, as this laſt number 3,65, or $3\frac{65}{100}$ ſhews more immediately the goodneſs of the air in queſtion, as I have juſt obſerved, by ſuppoſing with Dr. INGEN-HOUSZ the meaſure of the eudiometer to be divided into 100 equal parts, it will be $100a + 400n = 135$, and 365, expreſſing the volume of the two airs deſtroyed, will become a whole number.

But, inſtead of writing $100a + 400n = 135$, &c. I ſhall continue to write $1a + 4n = 1{,}35$, and ſhall expreſs the laſt number (3,65) as a whole number notwithſtanding; and I ſhall ſometimes (following the example of Dr. INGEN-HOUSZ) write this number *only*, in noting the goodneſs of any air in queſtion.

I would juſt obſerve, with reſpect to the proceſs of proving the goodneſs of any kind of air, by the teſt

of

of nitrous air, that I mix the two airs in a phial, about 1 inch in diameter and 4 inches long, putting the air to be proved into the phial firſt, and then introducing the nitrous air, one meaſure after another, till the volume of the two airs, after the diminution has taken place, amounts to more than *one* meaſure, and is leſs than *two* meaſures.

Immediately after the introduction of each meaſure of nitrous air, I give the phial a couple of ſhakes; after which I ſuffer it to ſtand at reſt, while I prepare another meaſure of nitrous air, which commonly takes up about 20 ſeconds.

The meaſure of the eudiometer being filled with air, I ſuffer it to remain quiet under water 15 ſeconds, or while I can leiſurely count 30, in order that the air may have time to acquire the temperature of the water in the trough, and that the water in the meaſure may have time to run down from the ſides of the glaſs tube; and in ſhutting the ſlider of the eudiometer I take care to bring it to be exactly even with the ſurface of the water in the trough. Similar precautions are likewiſe made uſe of in meaſuring the volume of the two airs in the tube of the eudiometer, after they have been mixed and diminiſhed in the phial.

In order that I may know when I have added nitrous air enough, to the air in the phial, ſo that the volume of the two airs may amount to 1 meaſure, and may not be greater than 2 meaſures, there are two marks upon the phial, made with the point of a diamond, the one ſhewing 1 meaſure of my eudiometer, the other ſhewing 2 meaſures.

The tube of my eudiometer is half an inch in

diameter

diameter internally, and 1 measure occupies $3\frac{1}{4}$ inches in length upon it, and the measure itself is made of a piece of the same tube. Both the one and the other are ground with fine emery on the inside, in order to take off the polish of the glass, and by that means facilitate the running down of the water, which might otherwise hang in drops upon the inside of the tube, upon the introduction of air.

The nitrous air was always fresh made, and of the same materials, *viz.* fine copper wire, dissolved in smoking spirits of nitre, diluted with 5 times its volume of water; and all possible attention was paid to every other circumstance that could contribute to the accuracy of the experiments.

I have thought it necessary to mention these particulars on account of the great difference in the apparent goodness of any kind of air proved by the test of nitrous air, which arises from the difference of the circumstances under which the experiments are made.

But to return to my experiments upon the air produced upon exposing silk, in water, to the action of the sun's rays.

Experiment, N° 2.

Finding that the quantity and the quality of the air produced, depended, in a great measure, upon the intensity of the light by which the water and the silk were illuminated, I was desirous of seeing whether, by depriving them entirely of all light, they would not at the same time be deprived of the power of furnishing air. To ascertain this fact, I took a

globe A, ſimilar to that made uſe of in the foregoing experiment, and having filled it with freſh ſpring-water, I introduced into it 30 grains of raw ſilk, and placing it with its cylindrical neck inverted in a jar filled with the ſame water, I covered the whole with a large inverted earthen veſſel, and expoſed it, ſo covered up, for ſeveral days, in my window, by the ſide of another globe B, containing a like quantity of water and ſilk, which I left naked, and conſequently expoſed to the direct rays of the ſun.

The reſult of this experiment was, that the water and ſilk in the globe expoſed to the ſun's rays furniſhed air in great abundance, as in the experiment before mentioned; while that in the globe covered up in darkneſs, produced only a few very inconſiderable air-bubbles, which remained attached to the ſilk.

Experiment, N° 3.

To ſee if heat would not facilitate the production of air in the globe ſheltered from the light, I now removed it from the window, and placed it near a German ſtove, where I kept it warmed to about 90° of FAHRENHEIT's thermometer for more than 24 hours; but this was all to no purpoſe. The air produced was ſo exceedingly ſmall in quantity, that it could neither be proved, nor meaſured, there being only a few detached air-bubbles, which had collected themſelves near the top of the globe.

The mean heat of the water in the globe expoſed in the ſun's rays, at the time when it furniſhed air in the greateſt abundance, was about 90° FAHRENHEIT. It was ſometimes as high as 96°; but air

was

was frequently produced in conſiderable quantities when the heat did not exceed 65° and 70°.

Experiment, N° 4.

Finding by the laſt experiment (N° 3.) that heat alone, without light, was not ſufficient to enable ſilk in water to produce air, I was deſirous of ſeeing the effect of light, without heat, upon them. To this end, I took the globe B, with its contents, and plunging it into a mixture of ice and water brought it to the temperature of about 50° F. and taking it out of this mixture, and expoſing it immediately in the ſun's rays (which were very piercing at the time) I entertained it in this temperature above two hours by the occaſional application of cloths, wet in ice water, to the lower part of the globe.

Notwithſtanding this degree of cold, a conſiderable quantity of air was produced; though it was not furniſhed in ſo great abundance as when the globe was ſuffered to become hot in the ſun's rays.

Having thus aſcertained the great effect of the ſun's rays in the production of the air furniſhed upon expoſing ſilk, in water, to their influence, my next attempt was to determine, whether this aroſe from any peculiar quality in the ſun's light; or whether *other light* would not produce the ſame effect. With a view to aſcertain this point, which I conceived to be of very great importance, I made the following intereſting experiment.

Experiment, N° 5.

Having removed all the air from the globe B,

and having ſupplied its place with a quantity of freſh water, ſo as to render it quite full, I replaced it, inverted, in its jar, and removing it into a dark room, ſurrounded it with 6 lamps, with reflectors, and 6 wax-candles, placed at different diſtances from 3 to 6 inches from it, and ſo diſpoſed as to throw the greateſt quantity of light poſſible upon the ſilk in the water, taking care at the ſame time that the water ſhould not acquire a greater heat than that of about 90° F.

Things had not remained in this ſituation above 10 minutes, when I plainly diſcovered the air-bubbles beginning to make their appearance upon the ſurface of the ſilk; and at the end of 6 hours, there was collected at the upper part of the globe a quantity of air ſufficient to make a proof of its goodneſs with nitrous air; and, upon trial, I had the pleaſure to find, that it was *dephlogiſticated*, and of ſuch a degree of purity, that 1 meaſure of it with 3 meaſures of nitrous air occupied no more than 1,68 meaſure.

I afterwards expoſed, to the ſame light, in ſmall inverted glaſs jars, filled with water, a freſh-gathered healthy leaf of the peach tree, and a ſtem of the pea plant with 3 leaves upon it; and both theſe vegetables furniſhed air in the ſame manner as they are known to furniſh it when expoſed, under ſimilar circumſtances, to the action of the ſun's direct rays, but in leſs quantities, which I attribute to the greater intenſity of the ſun's light above that of my lamps.

The experiment with the ſilk and water I repeated ſeveral times, always with nearly the ſame reſult.

The

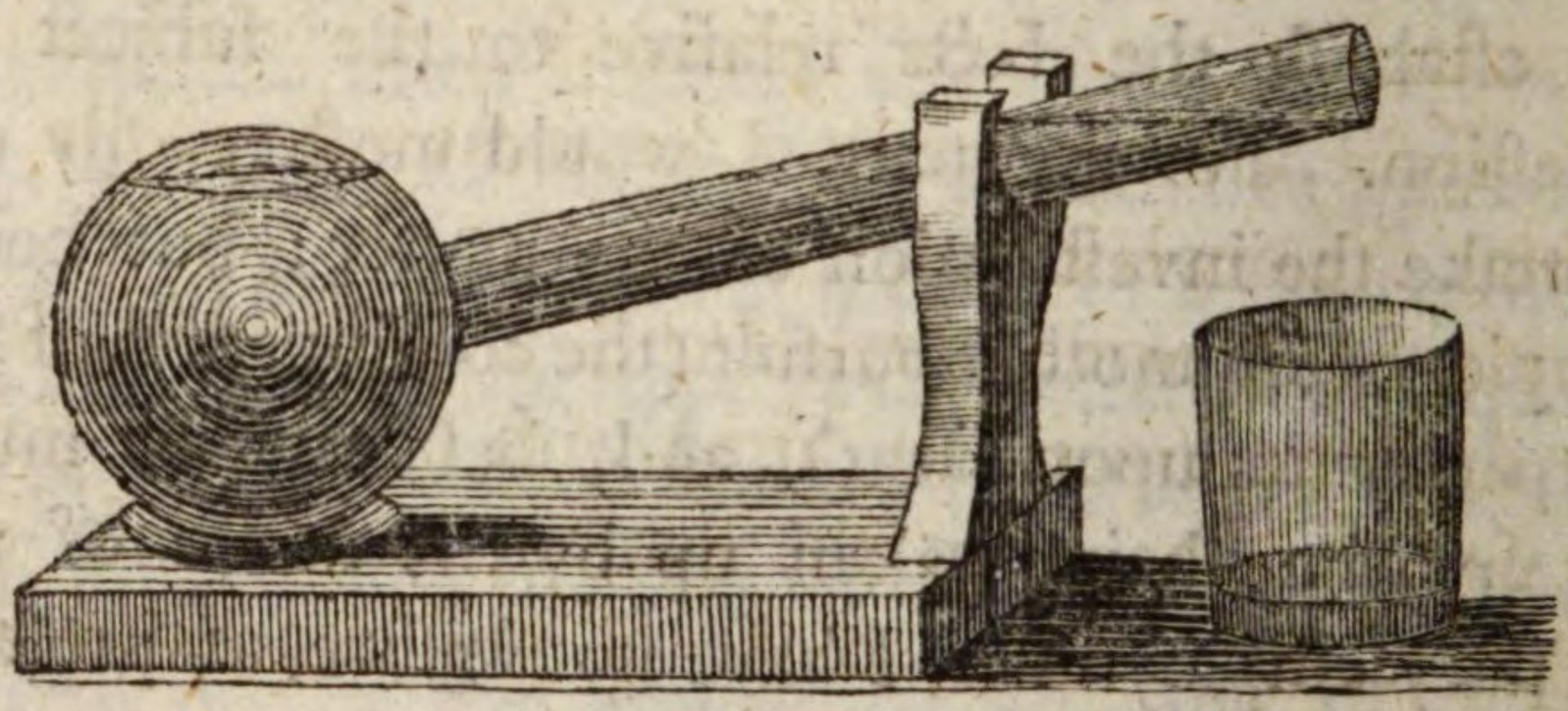
The quantity of air furniſhed was ſometimes a little greater, and ſometimes a little leſs; but it was always in much greater abundance than that furniſhed by an equal quantity of water and ſilk expoſed to the ſame heat, but excluded from the light; and I have reaſon to think, it was of a much ſuperior quality, though the quantity of that produced in the dark was too ſmall to be ſubmitted to any proof.

Theſe experiments appear to me to be of ſo much importance, that I could wiſh they might be repeated, and varied, in ſuch a manner as thoroughly to eſtabliſh the facts relative to the ſubject in queſtion. For my part, I would moſt readily undertake the inveſtigation of the matter; but being employed in another purſuit (the continuation of my Experiments upon Heat), and, beſides this, much of my time being taken up by the duties of my military employment, I have not leiſure, at preſent, for ſuch an undertaking.

Perhaps it may be proved by future experiment, that the matter of light is a conſtituent part of what is called pure or dephlogiſticated air; if ſo, may we not venture to conclude with M. SCHEELE, that the *light*, as well as the heat, produced by flame, and in general all burning bodies, ariſes *ſolely* from the decompoſition of this air, and not from the phlogiſton or inflammable principle of the body which is burnt? There are many phænomena which would ſeem to juſtify this opinion.

But to proceed in the account of my experiments.—The operation of inverting the globes under water, and placing them in the jars, and of diſ-

placing and replacing them upon removing the air produced, being attended with ſome inconveniences, I had recourſe to another method of diſpoſing of the apparatus, much more ſimple and more convenient. The globes being filled, were laid upon a ſmall piece of deal board, with their necks inclined at an angle of about 20° above the plane of the horizon, and ſupported in this poſition by a perpendicular fork of wood, fixed to the end of the board, as repreſented by the following figure. The part



of the board, upon which the under part of the globe repoſed, was hollowed a little, to prevent the globe from rolling; or, what I found more ſafe and convenient, a ſmall ring, or hoop, of ſoft wood, was nailed down upon the board in that part.

By this arrangement the jars were ſpared, and the end of the neck of the globe being eaſy to be come at, by introducing a wire, or, what I commonly made uſe of in preference, a ſmall glaſs tube, into the globe, the air hanging attached to the ſilk can at all times be ſeparated from it; which is often neceſſary, in order to determine with greater preciſion the quantity of air furniſhed in any given time.

The

The air produced, naturally riſes to that part of the globe which is uppermoſt, where it collects in a body, driving out an equal volume of water; which to prevent its running about, may be collected, by placing a proper veſſel under the mouth, or end of the neck of the globe, to receive it.

The method of removing the air from the globe is too well known to require a deſcription. I would however obſerve, that in doing it care ſhould be taken, that the water in which the globe is immerſed be quite clean, and of the ſame kind with that in the globe, otherwiſe that which enters the globe, to replace the air removed, might derange the experiment.

Having provided myſelf with a number of globes of different ſizes, all fitted with boards or ſtands to ſupport them, in the manner above deſcribed, I proceeded in the courſe of my experiments as follows.

Finding that raw ſilk, expoſed in water to the action of light, cauſes the water to yield pure air in ſo great abundance, I was deſirous of finding out whether this aroſe from any peculiar quality poſſeſſed by the ſilk; or whether other bodies might not be made to produce the ſame effect: to this end, having provided 6 globes, each about $4\frac{1}{2}$ inches in diameter, and having filled them with freſh ſpring-water, I introduced into them the following ſubſtances, and expoſed them all, at the ſame time, to the action of the ſun's rays.

In the globe N° 1. I put 15 grains of ſheep's wool,
N° 2.—— 15 grains of Eider down,
N° 3.—— 15 grains of the fine fur of a Ruſſian hare,
N° 4.—— 15 grains of cotton wool,
N° 5.—— 15 grains of lint, or the ravelings of fine linen,
N° 6.—— 15 grains of human hair; theſe ſubſtances being all well waſhed, and being thoroughly freed of air, by being wetted before they were put into the globes.

The reſults of theſe experiments were as follows.

Experiment, N° 6.

The globe N° 1. which contained the ſheep's wool, did not begin to furniſh air in any conſiderable quantity till the third day of its being expoſed to the action of the ſun's rays; and ſeveral days of cloudy weather intervening, I did not remove the air till the eighth day, when I collected 1¾ cubic inch, which, proved with nitrous air, gave $1a+3n=1{,}28$, or 272 degrees.

The wool at no time furniſhed more than one third part of the air, which an equal quantity of ſilk would have furniſhed under the ſame circumſtances.

The water was very faintly tinged of a greeniſh hue.

Experiment, N°. 7.

The water in the globe N° 2. with the Eider down, began almoſt immediately to furniſh air, and

continued to yield it during the whole time of the experiment, nearly in as large quantities as the water with ſilk had done in the former experiments, and nearly of the ſame quality. $1\frac{3}{4}$ cubic inches of this air furniſhed the eighth day from the beginning of the experiment, or the third of ſunſhine, proved with nitrous air, gave $1a+3n=1,34$ or 266 degrees of purity.

The water was faintly tinged of a greeniſh, yellowiſh caſt, and the Eider down, when examined attentively, appeared to be covered with a greeniſh ſlime.

Experiment, N° 8.

The globe N° 3. with the hare's fur (which was white) furniſhed more air than the ſheep's wool, but not ſo much as the Eider down. After four days of ſunſhine, I collected 2 cubic inches of this air, which, proved with nitrous air, gave $1a+3n=1,44$, or 256.

The water had acquired a very faint yellowiſh hue; but it did not appear to have loſt much of its tranſparency, or to be diſpoſed to depoſit any ſediment.

The air produced in this experiment made its appearance in a different manner from that furniſhed in thoſe preceding it, the air-bubbles which appeared upon the ſurface of the fur being at conſiderable diſtances from each other, and growing to an uncommon ſize before they detached themſelves to riſe to the ſurface of the water.

Experiment,

Experiment, N° 9.

The globe N° 4. with cotton wool, furniſhed a conſiderable quantity of air, which appeared to be better than that furniſhed by any of the five other globes. Proved with nitrous air, it turned out $+3n=1,07$, or 293; and what was particular, the water did not appear to have altered its colour in the leaſt, or to have loſt any thing of its tranſparency.

Experiment, N° 10.

The globe N° 5. with ravelings of linen, was very tardy in furniſhing air, and produced but a ſmall quantity; at the end of a fortnight, however, I collected about 2 cubic inches, which, proved with nitrous air, gave $1a+3n=1,51$, or 249.

The air appeared to have very little diſpoſition to fix itſelf to the ſurface of this ſubſtance. It was very ſeldom that there were air-bubbles enough attracted to it to cauſe it to riſe to the ſurface of the water, and the few bubbles which occaſionally made their appearance, very ſoon diſappeared upon the diminution of the light and heat of the ſun. In ſhort, it appeared, that there is but a very feeble attraction between this ſubſtance and the particles of air, at leaſt when they are diſſolved in water. Whether this ariſes from the ſuperior affinity of the ſubſtance to water, or not, I will not pretend to decide; but it appears to be probable, as there is ſo ſtrong an attraction between water and linen, or flax, which is apparent from the avidity with which a piece of dry linen drinks up that fluid, and

becomes

becomes wet, even to a conſiderable diſtance, when one end of it only is placed in water.

It muſt be remembered, that I here conſider the ſeparation of the air from water as a ſimple operation; and that I do not take into the account the purification, or rather the generation, of this air. Though there is great reaſon to conclude, that theſe two operations are very nearly connected; yet, to ſimplify my inquiries, I ſhall, in the firſt place, conſider the appearances as they preſented themſelves to my ſenſes. It will be eaſy afterwards to draw any concluſions from the reſults of the experiments, which a careful examination and compariſon of the various phænomena will juſtify.

Experiment, N° 11.

The globe N° 6. with human hair, furniſhed ſtill leſs air than that with ravelings of linen in the laſt mentioned experiment; but, notwithſtanding the ſmallneſs of the quantity, it was conſiderably ſuperior in quality to atmoſpheric air, for, proved with nitrous air, it gave $1a+2n=1{,}45$, or 155; whereas common air, proved at the time, gave $1a+1n=108$, or 92.

Experiment, N° 12.

To aſcertain the relative goodneſs of the air furniſhed by the water in theſe experiments, and of that produced by expoſing freſh healthy vegetables, in water, to the action of the ſun's light, according to the method of Dr. INGEN-HOUSZ, I collected a ſmall quantity of air from a ſtem of a pea plant, which

which had four healthy leaves upon it, expoſed in water to the ſun's light, and found it to be much inferior to that furniſhed in the experiments with ſilk, and the various other ſubſtances I made uſe of. Proved with nitrous air, it gave $1a+2n=1{,}05$, or 195.

An entire plant of houſewort, of a moderate ſize, expoſed in 12 ounces of water 7 hours, to the action of the ſun's rays, at a time when the weather was remarkably fine, and very hot, furniſhed about $\frac{3}{4}$ of a cubic inch of air, which was ſo much worſe than common air, that 1 meaſure of it, with 1 meaſure of nitrous air, occupied 1,36 meaſures; or it was $1a+1n=1{,}36$, or 64. But I lay no kind of ſtreſs upon the reſult of this experiment, as it is more than probable, that the badneſs of the air aroſe from the roots of the plants; for, from the leaves alone, I have frequently ſince obtained air, which appeared to be conſiderably better than common air.

From the leaves of the peach-tree, I obtained an air, which, proved with nitrous air, gave $1a+2n=1{,}32$, or 168; but I did not think it neceſſary to multiply theſe experiments, particularly as Dr. INGENHOUSZ and Mr. SENNEBIER have given us the reſults of ſo many of theirs upon the ſame ſubject, of the accuracy of which there is no room left to doubt. I ſhall therefore content myſelf with referring to the reſults of their experiments.

With a view to determining, with greater preciſion, the quantity and the quality of the air produced by a given quantity of water, and of ſilk, expoſed

for

for a given time to the action of the sun's rays, I made the following experiment.

Experiment, N° 13.

A globe of fine, clear, white glass, about $8\frac{5}{10}$ inches in diameter, and containing 296 cubic inches, being filled with fresh spring-water, and 30 grains of raw silk, was exposed in my window three days, *viz.* 12th, 13th, and 14th of May last, (1786) these days being for the most part cold and cloudy, with short intervals of sunshine. Air produced $9\frac{1}{2}$ cubic inches; quality $1a+3n=1,61$, or 239.

May 15. This air being removed, and its place supplied with fresh water, the globe exposed in the sun this day from nine o'clock in the morning till five o'clock in the afternoon, the weather being very fine, yielded $8\frac{46}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a+4n=1,74$, or 326. The heat of the water in the globe, during the experiment, was from 70° to 98° F. The water had now lost considerably of its transparency, and had assumed a light greenish hue.

May 16. The air furnished yesterday being removed, the globe furnished this day, during six hours of sunshine, 9 cubic inches of air, which, proved with nitrous air, gave $1a+4n=1,44$, or 356.

May 17. The globe furnished this day, during $3\frac{1}{2}$ hours of sunshine, 6 cubic inches of air, of a very eminent quality; for, proved with nitrous air, it gave $1a+4n=1,35$, or 365.

May 18. This day cold and cloudy; not more

than

than 1½ hours ſunſhine; air produced ¾ of a cubic inch; quality $1a+4n=1,56$, or 344.

May 19. The globe appearing now to be quite exhauſted of air, ſhewing no ſigns of furniſhing any additional quantity, though expoſed to the action of a very bright ſun, I removed the globe from the window, and placed it by the ſide of a German ſtove, where it was kept warm to 100° F. from 10 o'clock in the morning till 5 o'clock in the afternoon. By this means I obtained ¼ of a cubic inch of air, which, proved with nitrous air, gave $1a+4n=1,74$, or 326.

Not being able to obtain any more air from the globe, I now put an end to the experiment.

The quantities and qualities of the airs furniſhed upon the different days were as follows:

	Quantity.	Quality.
Upon the 12th, 13th, and 14th of May	9½ cubic inches	$1a+3n=1,61$, or 239
15th	$8\frac{46}{100}$	$1a+4n=1,74$, or 326
16th	9	$1a+4n=1,44$, or 356
17th	6	$1a+4n=1,35$, or 365
18th	¾	$1a+4n=1,56$, or 344
19th	¼	$1a+4n=1,74$, or 326
Total quantity	$33\frac{96}{100}$	mean qual. $1a+4n=1,84$, or 316

As in this experiment the air furniſhed each day was removed at night, and the place it occupied in the globe ſupplied with freſh water, I was deſirous of ſeeing what variation it would occaſion in the reſult of the experiment, if inſtead of removing the air from time to time, I ſuffered it to remain in the globe till the end of the experiment: to this end I made

Experiment,

Experiment, N° 14.

In which the globe being filled with fresh water, and the silk used in the last experiment (being first well washed), the whole was exposed four days to the action of the sun's rays, the weather being remarkably fine, and very hot. Upon removing the air produced, I found it amounted to $30\frac{1}{10}$ cubic inches; and its quality, proved with nitrous air, was $1a+3n=1{,}02$, or 298.

I should have continued the experiment for some days longer, as the globe did not appear to be exhausted; but the quantity of air already collected in the globe was so great that it became very difficult to remove it, without running the risk of losing a part of it, or of letting the air of the atmosphere enter the globe, either of which events would of course have spoiled the experiment. For safety therefore, and that I might not by an accident lose the trouble I had already had with it, I put an end to the experiment at the end of the fourth day.

The water had lost of its transparency, and had acquired a greenish cast, as in the last experiment; and in both these experiments I observed, that a considerable quantity of whitish yellowish earth was precipitated from the water, which falling to the bottom of the globe, attached itself to the glass in such a manner that it was with difficulty that it could be removed. These were general appearances, and took place in all cases, in a greater or less degree, where a considerable quantity of pure air was separated from water by the influence of light.

Experiment,

Experiment, N° 15.

The ſilk made uſe of in the laſt experiment having been frequently uſed in the foregoing experiments, I was deſirous of ſeeing the effect of making uſe of freſh ſilk; and alſo of varying the proportion between the quantity of ſilk, the quantity of water, and the ſize of the globe; accordingly, at 6 o'clock P. M. upon the 13th of June, I filled a ſmall globe, about three inches in diameter, or (to aſcertain its ſize more exactly) which contained juſt 20 cubic inches, with freſh ſpring-water, and 17 grains of raw ſilk, wound in a ſingle thread, which had never been put into water, or otherwiſe uſed, ſince it came out of the hands of the ſilk-winder.

At the end of four days, *viz.* the 14th, 15th, 16th, and 17th of June, this globe had only furniſhed $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a+1n=1{,}32$, or 68; conſequently was much worſe than common air.

Upon the 18th, it began to produce good air, and during ſix hours of ſunſhine it furniſhed $1\frac{15}{100}$ cubic inches, which, proved with nitrous air, gave $1a+3n=1{,}15$, or 285.

The two following days (*viz.* the 19th and 20th of June) it furniſhed $1\frac{27}{100}$ cubic inch of air, which, proved with nitrous air, gave $1a+3n=1{,}37$, or 263; after which it totally ceaſed to yield air, though expoſed for ſeveral days in the ſun's rays.

Total quantity of air produced $2\frac{67}{100}$ cubic inches; mean quality $1a+3n=1{,}46$, or 254.

By

By this experiment it appears, that raw ſilk, when uſed for the firſt time, does not immediately diſpoſe the water to yield pure air; on the contrary, that it phlogiſticates the air yielded by water to a very conſiderable degree; and this I afterwards found to be the caſe with ſeveral other ſubſtances.

Though the quality, at a medium, of the air furniſhed in this experiment was not quite ſo good as that furniſhed in the two experiments laſt mentioned (*viz.* N° 13. and N° 14.), yet its quantity, in proportion to the quantity of water made uſe of, was greater than in either of them: it amounted to ſomething more than *one-eighth* of the volume of the water.

Of all the ſubſtances I had hitherto made uſe of in theſe experiments, raw ſilk had furniſhed the greateſt quantity of pure air; or, to expreſs myſelf more properly, had cauſed the water to furniſh the greateſt quantity; but it appeared to me very probable, that ſome other body might be found, that poſſeſſed this property in a ſtill greater degree than ſilk. Turning this matter in my mind, it occurred to me, to make the experiment with the ſilky, or rather cotton-like, ſubſtance produced by a certain ſpecies of the Poplar-tree, *Populus nigra*, very common in this country, (Bavaria), and which, I believe, grows in England. I recollected that examining it ſome time before, with a different view (that of ſeeing if it might not be made uſe of with advantage, as a ſubſtitute for Eider down), and endeavouring to render it very dry, by expoſing it in

a china plate, over a chafing-diſh of hot embers, when it had acquired a certain degree of heat, ſmall parcels of it quitted the plate of their own accord, and mounted up to the top of the room.

This convinced me at the time, not only of its extreme fineneſs, but alſo of the ſtrong attraction which ſubſiſts between it and the particles of air; and it now occurred to me, that theſe qualities not only render it peculiarly proper as a ſubſtitute for Eider down, for confining heat, but likewiſe are properties of all others the moſt neceſſary to its ſupplying the place of ſilk in the production of air, by expoſing it in water to the action of the ſun's rays. I therefore loſt no time in making the following experiments.

Experiment, N° 16.

The large globe (contents 296 cubic inches) being filled with freſh ſpring-water, and 120 grains of poplar cotton, upon the evening of the 9th of June, and being the next day, the 10th of June, expoſed to the ſun, about four hours, upon the morning of the 11th the air produced was removed, and its quantity was found to be $1\frac{3}{4}$ cubic inch. Its quality was very bad, *viz.* $1a+1n=1{,}65$, or 35 degrees only better than thoroughly phlogiſticated air (azote).

Upon the 11th, 12th, and 13th, 1 cubic inch of air only was produced, and this appeared to be as bad as poſſible; for, proved with nitrous air, it gave $1a+1n=2$, or 0.

Upon the 14th, a few air-bubbles only were furniſhed;

niſhed; but, notwithſtanding theſe unfavourable appearances, I ſtill continued the experiment, and my patience was amply rewarded; for the next day, the 15th, the ſun being very powerful, and the weather very hot, the water changing ſuddenly to a greeniſh colour, began all at once to give good air in great abundance. In the courſe of the day $10\frac{42}{100}$ cubic inches were produced, which, proved with nitrous air, gave $1a+3n=1,43$, or 257.

June 16th, a very warm clear day. The globe expoſed in the ſun, from 8 o'clock in the morning till 5 o'clock in the afternoon, furniſhed $14\frac{34}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a+3n=1,34$, or 266.

June 17th, cloudy, with intervals of ſunſhine. The globe with about four hours ſun gave $7\frac{34}{100}$ cubic inches of air, of a very eminent quality, *viz.* $1a+4n=1,40$, or 360.

The water having by degrees loſt its tranſparency, and having acquired a deep green colour, it broke up this day, and depoſited a green ſediment; after which it recovered its tranſparency, and became almoſt colourleſs. It continued, notwithſtanding, to furniſh air in conſiderable quantities.

June 18th, being expoſed in the ſun's rays from 8 o'clock in the morning till 2 o'clock in the afternoon (when the heavens became overcaſt), the globe yielded $6\frac{27}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a+4n=1,44$, or 356.

June the 19th and 20th. Theſe two days the globe furniſhed no more than $3\frac{13}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a+3n=1,06$,

or 294; after which it ceaſed totally to furniſh air, and the colour of the water changed to a dead yellowiſh caſt, and the cotton aſſumed the ſame hue.

The following are the quantities and qualities of the different parcels of air furniſhed in the courſe of this experiment.

	Quantity.	Quality.
Upon the 10th of June	$1\frac{3}{4}$ cubic inches	$1a+1n=1,65$, or 35
11th, 12th, and 13th	1 - -	$1a+1n=2$, or 0
14th	0 -	
15th	$10\frac{42}{100}$ -	$1a+3n=1,43$, or 257
16th	$14\frac{34}{100}$ -	$1a+3n=1,34$, or 266
17th	$7\frac{34}{100}$ -	$1a+4n=1,40$, or 360
18th	$6\frac{27}{100}$ -	$1a+4n=1,44$, or 356
19th and 20th	$3\frac{13}{100}$ - -	$1a+3n=1,06$, or 294
Total quantity	$44\frac{1}{4}$ Mean quality	$1a+3n=1,23$, or 277

This experiment was repeated, and with nearly the ſame reſult; the total quantity of air produced being $41\frac{1}{3}$ cubic inches, and its quality, at a medium, $1a+3n=1,26$, or 274.

To aſcertain the relative fineneſs of this poplar cotton, and the thread of raw ſilk as ſpun by the worm, in order to make an eſtimate of the ſurface of the former, I examined them both at the ſame time under an excellent microſcope, when the diameter of the cotton, that is to ſay, of a ſingle thread or fibre of it, appeared to be not more than half as great as the diameter of the ſilk, conſequently its diameter was not more than $\frac{1}{3648}$ part of an inch; for I have found by experiment, that the diameter of a thread of ſilk, as ſpun by the worm, is only $\frac{1}{1824}$ of an inch.

The

The ſpecific gravity of the cotton I found to be very nearly equal to that of water, conſequently it is to that of ſilk as 1000 to 1734; its ſurface, therefore, is to the ſurface of an equal weight of raw ſilk in the compound proportion of 2 to 1, and of 1734 to 1000; that is to ſay, as 3468 to 1000.

Now, as the ſurface of 30 grains of raw ſilk amounts to 476 ſquare inches, the ſurface of 30 grains of poplar cotton muſt amount to 1651 ſquare inches, which gives 55 ſquare inches of ſurface for each grain in weight; conſequently the ſurface of the cotton made uſe of in the foregoing experiment (N° 16.) did not amount to leſs than 6600 ſquare inches; (for 120 grains, the weight of the cotton, multiplied by 55, gives 6600); an enormous ſurface indeed for a body, whoſe *ſolid contents* did not amount to quite half a cubic inch.

From hence it appears evidently, that the quantities of air furniſhed by water, in the experiments with raw ſilk, and with poplar cotton, were neither in proportion to the quantities of theſe ſubſtances made uſe of, nor to the quantities of their ſurfaces. It appears likewiſe, from the two laſt experiments, that the air which is furniſhed in the beginning of the experiment, or when the water is firſt expoſed to the action of the ſun's rays, is neither ſo good, nor in ſo great abundance, as afterwards, at a more advanced period; and that it totally ceaſes to be produced after a certain time.

To aſcertain, with greater preciſion, the qualities

of the air furniſhed at different periods of the experiment, or rather the period when the water begins to give good air; and alſo to determine the relative quantities and qualities of the airs produced in the experiments with raw ſilk, and in thoſe with poplar cotton, I made the following experiments.

Experiment, N° 17.

A globe, about 4½ inches in diameter, containing juſt 46 cubic inches, being filled in the evening with freſh ſpring-water, and 30 grains of raw ſilk, which had been previouſly waſhed thoroughly to free it of air and the remains of former experiments, and being expoſed the next day in my window, the weather being cold and cloudy, with not more than one hour of ſun-ſhine, ¼ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a+2n=1,86$, or 114.

The two following days, the weather being clear and moderately warm, 3¼ cubic inches of air were produced, which, proved with nitrous air, gave $1a+3n=1,14$, or 296.

Experiment, N° 18.

The globe having been again filled with freſh ſpring-water, and the ſame ſilk which had ſerved in the laſt experiment, after 2 nights, and 1 day of about 4 hours ſun, it had furniſhed 1⅛ cubic inch of air, whoſe quality was $1a+2n=1,13$, or 197.

The two following days, the weather being very fine, it furniſhed $3\frac{8}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a+4n=1,58$, or 342.

Experiment,

Experiment, N° 19.

The globe being again filled with fresh water, and the same silk well washed, and being exposed 2 days in the sun, it gave $2\frac{2}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a+3n=1,67$, or 233.

Experiment, N° 20.

A like globe, with fresh water, and an equal quantity of poplar cotton which had been used in former experiments, being exposed at the same time (*viz.* when the last experiment was made), gave $2\frac{53}{100}$ cubic inches of air, whose quality was $1a+3n=1,20$, or 280.

Experiment, N° 21.

A small globe, (contents 20 cubic inches,) with 17 grains of raw silk, exposed at the same time, give 1 cubic inch of air, which turned out $1a+3n=1,37$, or 263.

Experiment, N° 22.

A large globe, containing 296 cubic inches, being filled with fresh water and a small quantity of *conferva rivularis*, and exposed at the same time with the three globes abovementioned, gave $1\frac{1}{2}$ cubic inch of air, which, proved with nitrous air, gave $1a+2n=1,76$, or 124.

The water in this experiment was changed to a brown colour, owing, as I conceived, to the too great heat the *conferva* acquired in the sun.

These experiments were made between the 2d and the 5th of July, (1786).

Experiment, N° 23.

Surprised at the smallness of the quantity, and the inferior quality, of the air produced in the last experiment, I was induced to repeat it; accordingly, the globe being again filled with water, and a quantity of fresh *conferva rivularis* (a small handful), and being exposed to the action of the sun's rays during 3 fine days, $13\frac{34}{100}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a+3n=1{,}54$, or 246.

At the end of the experiment the water appeared to be very faintly tinged of a greenish cast.

The two following experiments were made upon the 20th and 21st of August.

Experiment, N° 24.

A globe about $4\frac{1}{2}$ inches in diameter (contents 46 cubic inches) being filled with fresh spring-water, and 30 grains of raw silk, which had been used in many preceding experiments, and being exposed to the action of the sun's rays two days, in all about 8 hours of sunshine, the weather being cloudy great part of the time, $1\frac{6}{100}$ cubic inch of air was produced, which, proved with nitrous air, gave $1a+3n$ 1,96, or 204.

Experiment, N° 25.

At the same time an equal globe, containing fresh spring-water, and about 15 grains of poplar cotton (which had likewise been used in former experiments), produced $1\frac{28}{100}$ cubic inch of air, which,

which, proved with nitrous air, gave $1a+3n=1,40$, or 260.

The water in both theſe experiments had acquired a faint greeniſh caſt; but the colour of that with the cotton was rather the deepeſt.

Upon examining this water under a microſcope, I found it contained a great number of *animalculi*, exceedingly ſmall, and of nearly a round figure. That with the ſilk contained them likewiſe, and of the ſame kind, but not in ſo great abundance. I never failed to find them in every caſe in which the water uſed in an experiment had acquired a greeniſh hue; and from their preſence alone, I think it more than probable, that the colour of the water, *in the firſt inſtance*, aroſe, in all caſes. I have ſpent a great deal of time in obſerving them, and have made many experiments upon their production; but as I have not yet been able to ſatisfy my own mind, with reſpect to the part they act in the operation of purifying the air in water, or generating pure air from it, I ſhall not add to the length of this paper, by giving an account of my inquiries and obſervations reſpecting them.

I was yet by no means ſatisfied with reſpect to the part which the ſilk and other bodies, expoſed in water in the foregoing experiments, acted in the generation of the air produced.

Dr. PRIESTLEY has long ſince diſcovered, that many animal and vegetable ſubſtances, putrefying, or rather diſſolving, in water, in the ſun, cauſe the water to yield large quantities of dephlogiſticated air; but I could hardly conceive, that the ſmall quantity of

ſilk

ſilk which was uſed in my experiments, and which had been conſtantly in water for more than three months, and had ſo often been waſhed, and even boiled in water, ſhould yet retain a power of communicating any thing to the large quantities of freſh water in which it was ſucceſſively placed; at leaſt *any thing* in ſufficient quantities to impregnate thoſe bodies of water, and to cauſe them to yield the great abundance of air which they produced.

It was ſtill more difficult to account for the pure air produced in the experiments with wool and fur, and human air; eſpecially, as in ſome of theſe experiments the water had not ſenſibly changed colour, nor did it appear to have loſt any thing of its tranſparency. It is true, in theſe caſes, the quantities of air produced were very ſmall; but yet its quality was better than that of common air, and conſiderably ſuperior to that of the air commonly exiſting in the water, previous to its being expoſed to the action of the ſun's light. In ſhort, it was dephlogiſticated in the experiment; but the *manner* in which this was done is very difficult to aſcertain.

With a view to throwing ſome new light upon this intricate ſubject, I made the following experiments.

Experiment, N° 26.

Concluding that if ſilk, and other bodies, uſed in the foregoing experiments, actually did not contribute any thing *conſidered as chemical ſubſtances*, in the proceſs of the production of pure air yield-

ed by water; but if, on the contrary, they acted merely as a mechanical aid, in the *separation* of the air from the water, by affording a convenient surface for the air to attach itself to; in this case, any other body, having a large surface, and attracting air in water, might be made use of instead of silk in the experiment, and pure air would be furnished, though the body so made use of should be totally incapable of communicating *any thing whatever* to the water.

To ascertain this fact, washing the great globe (containing 296 cubic inches) perfectly clean, and filling it with fresh spring-water, I introduced into it a quantity of the fine flexible thread of glass, commonly called *spun glass*, such as is used for making brushes for cleaning jewels, and for making a kind of artificial feather frequently sold by the Jew pedlars. This spun glass is no other than common glass, drawn out, when hot, into an exceeding fine thread; which thread, in consequence of its extreme fineness, retains its flexibility after it has grown cold.

I made choice of this substance, not only on account of its great surface, but also on account of the strong attraction which is known to subsist between glass and air, and the impossibility of its communicating any thing to the water.

The result of the experiment was, that the globe being exposed in the sun, air-bubbles began almost immediately to make their appearance upon the surface of the spun-glass, and in four hours $\frac{77}{100}$ of a cubic inch of air was collected, which, proved

with

with nitrous air, gave $1a+1n=1,12$, or 88; after which, not a ſingle air-bubble more was produced, though the globe was expoſed a whole week in the window, during which time there were ſeveral very warm, fine, ſunſhiny days.

This experiment ſhews evidently, that ſomething more is wanting to the production of pure air by water, expoſed in the ſun, than merely a ſurface to which the air diſſolved in the water can attach itſelf, in order to its making its eſcape.

The air furniſhed in this experiment was doubtleſs merely that with which the water, iſſuing from the earth, was overcharged, and which would have made its eſcape from the water, had the water, inſtead of being expoſed with the ſpun glaſs in the ſun, been ſimply left for ſome time expoſed to the free air of the atmoſphere.

It appears that this air, naturally exiſting in ſpring-water, inſtead of being dephlogiſticated, is ſomething worſe than common air; and this agrees with the obſervations of Dr. PRIESTLEY, and ſeems to juſtify his opinion with reſpect to the cauſe of the fertility of lands waſhed by waters iſſuing from the earth.

If the above experiment ſhews that ſomething is wanting to be mixed with water, in order to enable it to yield pure air, when expoſed to the action of the ſun's light, the following ſhew, that this *ſomething*, whatever it may be, is frequently to be found in the water itſelf, in its natural ſtate.

Experiment, N° 27.

A large jar of clear white glaſs, containing 455 cubic

cubic inches, being waſhed very clean, was filled with freſh ſpring-water, and inverted in a glaſs baſon of the ſame, and placed in the middle of the garden of the Elector's palace, at Munich, where it was left expoſed to the weather 28 days.

At the ſame time another like jar was filled with water, taken from a pond in the garden, in which many aquatic plants were growing, and was expoſed in the ſame place, and during the ſame period. This water had a very faint greeniſh caſt. The pond from which it was taken is fed by a large river (the Iſar), which runs by the town.

The ſecond day after theſe waters had been expoſed in the ſun, I obſerved, that a ſmall quantity of air had collected itſelf at the upper part of each of the jars.

The third, fourth, and fifth days, the pond water furniſhed air in pretty large quantities; and it went on to yield it without intermiſſion, when the ſun ſhone upon it, till the fourteenth day, when it ſeemed to be nearly exhauſted. I continued the experiment, however, till the twenty-eighth day, though during the laſt fortnight the quantity of air in the jar did not appear to be ſenſibly increaſed.

The ſpring-water, during the firſt five or ſix days, furniſhed very little air; and it was not till the fourteenth day that it began to yield it in any conſiderable quantities. From this time it went on to furniſh it, though but very ſlowly till about the twenty-ſecond day, when it ceaſed, appearing to be quite exhauſted.

Upon

Upon the twenty-eighth day I removed the airs from the jars, when I found their quantities and qualities to be as follows:

	Quantity.	Quality.
Air furniſhed by the ſpring-water.	14 cubic inches	$1a+2n=1,62$, or 138
by the pond-water	$31\frac{1}{2}$ - - -	$1a+3n=1,48$, or 252

Neither the colour of the ſpring-water, nor that of the pond-water, appeared to be ſenſibly changed; but both the one and the other of theſe waters had depoſited a conſiderable quantity of earth, which was found adhering to the ſurfaces of the glaſs baſons in which the jars were inverted.

As theſe baſons were rather deep, and as they were very thick in glaſs, and conſequently not very tranſparent, their bottoms, where the ſediment of the water was collected, were, in a great meaſure, obſcured, or deprived of the direct rays of the ſun. Suſpecting that this circumſtance might have had ſome effect, ſo as to have hindered the water from furniſhing ſo much air as otherwiſe it might have yielded, to ſatisfy myſelf reſpecting this matter I repeated the experiment, diſpoſing the apparatus in ſuch a manner, that the ſediment of the water, which attached itſelf to the bottom of the veſſel in which the jar was inverted, had the advantage of being perfectly illuminated.

Experiment, N° 28.

In a large cylindrical jar, of very fine tranſparent glaſs,

glass, 10 inches in diameter, and 12 inches high, filled with spring water, I inverted a conical glass jar, $9\frac{3}{4}$ inches in diameter at the bottom, and containing 344 cubic inches, filled with the same water; and exposed the whole 21 days, in a window fronting the south.

The quantity of air produced amounted to 40 cubic inches; and its quality, proved by the test of nitrous air, gave $1a+3n=1,87$, or 213.

The water in this experiment furnished very little air till the seventh day; but after that time, having assumed a faint greenish cast, and a fine greenish slimy sediment (the *green matter* of Dr. PRIESTLEY) beginning to be formed upon the bottom of the jar, it began to yield air in abundance, and continued to furnish it in pretty large quantities till about the eighteenth day, when it appeared to be exhausted.

Why the water should turn green in this experiment, and not in the last, I know not; unless it was in consequence of the large surface of water in the cylindrical jar, which was exposed to the air in this experiment; or in consequence of the sun's shining directly upon the bottom of the vessel where the sediment was formed.

In the former experiment the bason in which the jar was inverted was but just big enough to admit the jar; and as the jar was cylindrical, the surface of the water exposed to the atmosphere, in the bason, was but very small; and the bason being very thick, and formed of glass which, though of the white kind, was of an inferior

quality,

quality, and very imperfectly tranſparent, as I have already obſerved, the bottom of the baſon where the ſediment was formed, was but very imperfectly illuminated.

Having never been thoroughly ſatisfied with reſpect to the origin of the dephlogiſticated air produced upon expoſing freſh vegetables in water to the action of the ſun's rays, according to the method of Dr. INGEN-HOUSZ, my doubts, with reſpect to the opinion generally entertained of its being *elaborated* in the veſſels of the plant, inſtead of being removed, were rather confirmed by the reſult of theſe experiments; and however diſpoſed I was to adopt the beautiful theory of the purification of the atmoſphere by the vegetable kingdom, I was not willing to admit a fact which has been brought in ſupport of it, till it ſhould appear to me to have been proved by the moſt deciſive experiments.

That the freſh leaves of certain vegetables, expoſed in water to the action of the ſun's rays, cauſe a certain quantity of pure air to be produced, is a fact which has been put beyond all doubt; but it does not appear to me to be by any means ſo clearly proved, that this air is "*elaborated* in the "plant by the powers of vegetation;"—"phlogiſ-"ticated, or fixed air being firſt abſorbed, or im-"bibed by the plant as food, and the dephlogiſ-"ticated air being rejected as an excrement:" for, beſides that many other ſubſtances, and in which no elaboration, or circulation, can poſſibly be ſuſpected to take place, cauſe the water in

which

which they are expoſed to the action of light to yield dephlogiſticated air as well as plants, and even in much greater quantities, and of a more eminent quality, the circumſtances of the leaves of a vegetable, which, accuſtomed to grow in air, are ſeparated from its ſtem, and confined in water, are ſo unnatural, that I cannot conceive that they can perform the ſame functions in ſuch different ſituations.

Among many facts which have been brought in ſupport of the received opinion of the *elaboration* of the air in the veſſels of the plants in the experiments in queſtion, there is one upon which great ſtreſs has been laid, which, I think, requires further examination.

The freſh healthy leaves of vegetables, ſeparated from the plant, and expoſed in water to the action of the ſun's rays, appear, by all the experiments which have hitherto been made, to furniſh air *only for a ſhort time;* after a day or two, the leaves changing colour, ceaſe to yield air: and this has been conceived to ariſe from the powers of vegetation being deſtroyed; or, in other words, *the death of the plant;* and from hence it has been inferred, with ſome degree of plauſibility, not only that the leaves actually retained their vegetative powers for ſome time after they were ſeparated from their ſtock, but alſo, that it was in conſequence of the exertion of theſe powers that the air yielded, in the experiment, was produced.

But I have found, that though the leaves, expoſed in water to the action of light, actually do

ceaſe to furniſh air, after a certain time, yet that they *regain* this power after a ſhort interval, when they furniſh (or rather cauſe the water to furniſh) more and better air than at firſt, which can hardly be accounted for upon the ſuppoſition that the air is *elaborated* in the veſſels of the plant.

Experiment, N° 29.

A globe, containing 46 cubic inches, filled with freſh ſpring-water and two peach leaves, was expoſed in the window to the action of the ſun's rays, 10 days ſucceſſively (the weather being in general fine), when the following appearances took place.

The 1ſt and 2d day, a certain quantity of air was produced, about as much as in former like experiments. The 3d day very little was produced; and the 4th day none at all, the globe to all appearance being quite exhauſted. Continuing the experiment, however, upon the 5th day, the water having acquired a faint greeniſh hue, air was again produced pretty plentifully, *making its appearance upon the ſurface of the leaves in the form of air-bubbles, as at the beginning of the experiment*; at the end of the 6th day the air was removed, and it was found to amount to $\frac{54}{100}$ of a cubic inch, its quality being 232 degrees, or $1a+3n=1,68$.

Upon the 7th day $\frac{9}{10}$ of a cubic inch of air was produced of 297 degrees, or $1a+3n=1,03$; and,

During the 8th, 9th, and 10th days, $1\frac{3}{4}$ cubic inch of air, of 307 degrees (or $1a+4n=1,93$), was furniſhed; after which an end was put to the experiment.

Total

Total quantity of air produced $3\frac{19}{100}$ cubic inches; mean quality 291 degrees, or $1a+3n=1,09$.

Finding that *leaves which were dead*, or in which all the powers of vegetation were evidently destroyed, continued notwithstanding, to separate air from water, and that in so great abundance, I was desirous of seeing the effect of exposing fresh healthy leaves in water which I knew to be previously saturated with, and disposed to yield, dephlogisticated air. I conceived, that if the plants exposed in water actually imbibed fixed, or phlogisticated air as food, and after digesting it, "discharged the "dephlogisticated air as an excrement;" in that case, as there is no instance of any plant or animal, being able to nourish itself with its own excrement, the leaves exposed in water saturated with dephlogisticated air, instead of imbibing and elaborating it, would immediately die.

The experiments which I made to ascertain this fact, and which, without any comment, I shall submit to the consideration of the reader, were as follows.

Experiment, N° 30.

Having provided a quantity of water, which, by being exposed with a few green leaves in the sun, had acquired a greenish cast, and which I found was disposed to yield dephlogisticated air in great abundance, I filled a globe, containing 46 cubic inches, with this water, and, putting to it two healthy peach leaves, exposed the globe in the sun upon the 7th of September, from 11 o'clock in the morning till 2 o'clock in the afternoon (3 hours), when $\frac{7}{10}$ of a

cubic inch of air was produced, which, proved with nitrous air, gave $1a+3n=1,52$, or 248 degrees.

A like globe, with freſh ſpring-water and two peach leaves, expoſed at the ſame time, furniſhed only $\frac{1}{9}$ of a cubic inch of air, which, on account of the ſmallneſs of its quantity, I did not ſubmit to the teſt of nitrous air.

Experiment, N° 31.

September 8. Very fine clear weather, but rather cold for the ſeaſon. Three equal globes, A, B, and C, containing each 46 cubic inches, were filled as follows, and expoſed in the ſun from 9 o'clock in the morning, till half an hour paſt 4 in the afternoon, when they were found to have produced air as under mentioned.

The globe A, filled with water, which, by being previouſly expoſed in the ſun for ſeveral days, with potatoes cut in thin ſlices, had turned green, furniſhed $\frac{9}{100}$ of a cubic inch of air of 299 degrees, or $1a+3n=1,01$. N. B. This water, before it was put into the globe, was ſtrained through two thicknefſes of very fine Iriſh linen.

The globe B, filled with the ſame green potatoe water (ſtrained as before), to which were added four middling-ſized peach leaves, furniſhed $2\frac{1}{2}$ cubic inches of air of 320 degrees, or $1a+4n=1,80$.

The globe C, filled with *freſh ſpring-water*, with four peach leaves, furniſhed $\frac{52}{100}$ of a cubic inch of air of 151 degrees, or which, proved with nitrous air, gave $1a+2n=1,49$.

To aſcertain the quantities and qualities of the airs

remaining in the different waters uſed in this experiment, putting the globes ſeparately over a chafing-diſh of live coals, and making the water boil, taking care to hold the globe in ſuch an inclined poſition as that the air ſeparated from the water might be collected in the upper part of the globe, the airs produced were as follows.

	Quantity.	Quality.
By the green water in the globe A, inch - - - - - - - -	$\frac{85}{100}$ of a cubic	280 deg.
By the green water in the globe B,	$\frac{31}{100}$ - -	241
By the ſpring water in the globe C,	$\frac{11}{100}$ - -	68

The waters in theſe experiments were made to boil but for a moment; otherwiſe, it is probable, more air might have been ſeparated from them.

Finding that freſh leaves, expoſed to the action of the ſun's rays, in water which had already turned green, cauſed pure air to be ſeparated from the water in ſo great abundance, I repeated the experiment, —only, inſtead of leaves, I now made uſe of a ſmall quantity of *conferva rivularis;* when I had nearly the ſame reſult as with the leaves.

To aſcertain the relative quantities and qualities of the airs yielded by the green water, when expoſed with freſh leaves, and when expoſed with raw ſilk; and alſo to aſcertain, at the ſame time, how long, leaves, expoſed in green water, retain their power of ſeparating air from it, I made

Experiment, N° 32.

Two equal globes, A and B (containing each 46 cubic inches), the former (A) filled with green

potatoe water, ſtrained through linen, and four peach leaves;—the latter (B) filled with the ſame potatoe water, ſtrained in like manner, and 17 grains of raw ſilk, were expoſed from Sunday noon, September 10th, till Monday evening; the weather being cold, with many flying clouds, in all about 6 or 7 hours ſun.

The airs produced were as follows.

	Quality.
By the globe A, with green water and 4 peach leaves. Quantity $2\frac{7}{10}$ cubic inches - - -	292 deg.
By the globe B, with green water and 17 grs. of raw ſilk Quantity $2\frac{7}{10}$ cubic inches - - -	307

Another globe (C), filled with green water *alone*, was expoſed at the ſame time; but it was broken by an accident before the experiment was completed.

The two globes (A and B) with their contents, being again expoſed from Tueſday noon till Thurſday evening, yielded air as follows.

	Quantity.	Quality.
The globe A, with the peach leaves	$4\frac{47}{100}$ cubic inches	344 deg.
The globe B, with raw ſilk - -	$4\frac{3}{10}$ - -	350

N. B. The weather on Tueſday and Wedneſday was cold, with very little ſunſhine; but Thurſday was a very fine warm day, when the greateſt part of the air was produced. This air was removed, and proved on Friday morning the 15th September.

Perhaps all the appearances above deſcribed might be ſatisfactorily accounted for, by ſuppoſing the air produced in the different experiments to have been generated in the maſs of water by the *green matter*; and that the leaves, the ſilk, &c. did no more than *aſſiſt it in making its eſcape*, by affording it a con-

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venient ſurface to which it could attach itſelf, in order to its collecting itſelf together, and taking upon itſelf its elaſtic form.

The phænomena might likewiſe be accounted for by ſuppoſing the *green matter* to be a vegetable ſubſtance, agreeable to the hypotheſis of Dr. PRIESTLEY, and that attaching itſelf to the ſurfaces of the bodies expoſed in the water, as a plant is attached to its ſoil, it grows; and, in conſequence of the exertion of its vegetative powers, the air yielded in the experiment is produced.

I ſhould moſt readily have adopted this opinion, had not a careful and attentive examination of the green water, under a moſt excellent microſcope, at the time when it appeared to be moſt diſpoſed to yield pure air in abundance, convinced me, that *at that period*, it contains nothing that can poſſibly be ſuppoſed to be of a vegetable nature. The colouring matter of the water is evidently of an animal nature, being nothing more than the aſſemblage of an infinite number of very ſmall, active, oval-formed *animalculi*, without any thing reſembling *tremella*, or that kind of *green matter*, or water moſs, which grows upon the bottom and ſides of the veſſel when this water is ſuffered to remain in it for a conſiderable time, and into which Dr. INGEN-HOUSZ ſuppoſes the animalculi above mentioned to be actually transformed.

But having finiſhed the account of my experiments, I ſhall finiſh this paper, not daring to venture conjectures upon a ſubject ſo intricate in itſelf, and ſo new, and upon which the ableſt philoſophers of the age ſeem to be ſo much divided in opinion.

AN

ACCOUNT

OF SOME

EXPERIMENTS

MADE TO DETERMINE

The QUANTITIES *of* MOISTURE ABSORBED *from the* ATMOSPHERE *by* VARIOUS SUBSTANCES.

[Read before the Royal Society, March 22, 1787.]

BEING engaged in a courſe of experiments upon the conducting powers of various bodies with reſpect to heat, and particularly of ſuch ſubſtances as are commonly made uſe of for cloathing, in order to ſee if I could diſcover any relation between the conducting powers of thoſe ſubſtances, and their power of abſorbing moiſture from the atmoſphere, I made the following experiments.

Having provided a quantity of each of the under mentioned ſubſtances, in a ſtate of the moſt perfect cleanneſs and purity, I expoſed them, ſpread out upon clean china-plates, twenty-four hours, in the dry air of a very warm room (which had been heated every day for ſeveral months by a German ſtove), the laſt ſix hours the heat being kept up to 85° of FAHRENHEIT's thermometer; after which I entered the room with a very accurate balance, and weighed equal quantities of theſe various ſubſtances, as expreſſed in the following Table.

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This being done, and each ſubſtance being equally ſpread out upon a very clean china plate, they were removed into a very large uninhabited room upon the ſecond floor, where they were expoſed 48 hours, upon a table placed in the middle of the room, the air of the room being at the temperature of 45° F.; after which they were carefully weighed (in the room) and were found to weigh as under mentioned.

They were then removed into a very damp cellar, and placed upon a table, in the middle of a vault, where the air, which appeared by the hygrometer to be completely ſaturated with moiſture, was at the temperature of 45° F.; and in this ſituation they were ſuffered to remain three days and three nights, the vault being hung round, during all this time, with wet linen cloths, to render the air as damp as poſſible, and the door of the vault being ſhut.

At the end of the three days I entered the vault, with the balance, and weighed the various ſubſtances upon the ſpot, when they were found to weigh as is expreſſed in the third column of the following Table.

The various ſubſtances.	Weight after being dried 24 hours in a hot room.	Weight after being expoſed 48 hours in a cold uninhabited room.	Weight after being expoſed 72 hours in a damp cellar.
	Pts.	Pts.	Pts.
Sheep's wool - -	1000	1084	1163
Beaver's fur - -	1000	1072	1125
The fur of a Ruſſian hare -	1000	1065	1115
Eider down - -	1000	1067	1112
Silk { Raw, ſingle thread	1000	1057	1107
Silk { Ravelings of white taffety	1000	1054	1103
Linen { Fine lint - -	1000	1046	1102
Linen { Ravelings of fine linen	1000	1044	1082
Cotton wool - -	1000	1043	1089
Silver wire, very fine, gilt, and flatted, being the ravelings of gold lace.	1000	1000	1000

N. B. The weight made uſe of in theſe experiments was that of Cologne, the *parts*, or leaſt diviſions, being = $\frac{1}{65536}$ part of a mark, conſequently 1000 of theſe *parts* make about $52\frac{3}{4}$ *grains Troy.*

I did not add the ſilver wire to the bodies above mentioned, from any idea that that ſubſtance could poſſibly imbibe moiſture from the atmoſphere; but I was willing to ſee whether a metal, placed in air ſaturated with water, is not capable of receiving a ſmall addition of weight from the moiſture attracted by it, and attached to its ſurface; from the reſult of the experiment, however, it ſhould ſeem that no ſuch attraction ſubſiſts between the metal I made uſe of, and the watery vapour diſſolved in air.

I was totally miſtaken in my conjectures relative to the reſults of the experiments with the other ſubſtances. As linen is known to attract water with

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ſo much avidity; and as, on the contrary, wool, hair, feathers, and other like animal ſubſtances, are made wet with ſo much difficulty, I had little doubt but that linen would be found to attract moiſture from the atmoſphere with much greater force than any of thoſe ſubſtances; and that, under ſimilar circumſtances, it would be found to contain much more water: and I was much confirmed in this opinion upon recollecting the great difference in the apparent dampneſs of linen and of woollen clothes, when they are both expoſed to the ſame atmoſphere. But theſe experiments have convinced me, that all my ſpeculations were founded upon erroneous principles.

It ſhould ſeem, that thoſe bodies which are the moſt eaſily wetted, or which receive water, in its unelaſtic form, with the greateſt eaſe, are not thoſe which in all caſes attract the watery vapour diſſolved in the air with the greateſt force.

Perhaps the apparent dampneſs of linen, to the touch, ariſes more from the eaſe with which that ſubſtance parts with the water it contains, than from the quantity of water it actually holds: in the ſame manner as a body appears hot to the touch, in conſequence of its parting freely with its heat, while another body, which is actually at the ſame temperature, but which withholds its heat with greater obſtinacy, affects the ſenſe of feeling much leſs violently.

It is well known, that woollen clothes, ſuch as flannels, &c. worn next the ſkin, greatly promote inſenſible perſpiration. May not this ariſe princi-

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pally from the ſtrong attraction which ſubſiſts between wool and the watery vapour which is continually iſſuing from the human body?

That it does not depend entirely upon the warmth of that covering, is evident; for the ſame degree of warmth, produced by wearing more cloathing of a different kind, does not produce the ſame effect.

The perſpiration of the human body being abſorbed by a covering of flannel, it is immediately diſtributed through the whole thickneſs of that ſubſtance, and by that means expoſed by a very large ſurface, to be carried off by the atmoſphere; and the loſs of this watery vapour, which the flannel ſuſtains on the one ſide, by evaporation, being immediately reſtored from the other, in conſequence of the ſtrong attraction between the flannel and this vapour, the pores of the ſkin are diſencumbered, and they are continually ſurrounded by a dry, warm, and ſalubrious atmoſphere.

I am aſtoniſhed, that the cuſtom of wearing flannel next the ſkin ſhould not have prevailed more univerſally. I am confident it would prevent a multitude of diſeaſes; and I know of no greater luxury than the comfortable ſenſation which ariſes from wearing it, eſpecially after one is a little accuſtomed to it.

It is a miſtaken notion, that it is too warm a cloathing for ſummer. I have worn it in the hotteſt climates, and in all ſeaſons of the year, and never found the leaſt inconvenience from it. It is the warm bath of a perſpiration confined by a linen ſhirt, wet with ſweat, which renders the ſummer heats of

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thetropical climates ſo inſupportable; but flannel promotes perſpiration, and favours its evaporation; and evaporation, as is well known, produces poſitive cold.

I firſt began to wear flannel, not from any knowledge which I had of its properties, but merely upon the recommendation of a very able phyſician (Sir RICHARD JEBB); and when I began the experiments of which I have here given an account, I little thought of diſcovering the phyſical cauſe of the good effects which I had experienced from it; nor had I the moſt diſtant idea of mentioning the circumſtance. I ſhall be happy, however, if what I have ſaid, or done, upon the ſubject, ſhould induce others to make a trial of what I have ſo long experienced with the greateſt advantage, and which, I am confident, they will find to contribute greatly to health, and conſequently to all the other comforts and enjoyments of life.

I ſhall then think theſe experiments, trifling as they may appear, by far the moſt fortunate, and the moſt important ones I have ever made.

With regard to the original object of theſe experiments, the diſcovery of the relation which I thought might poſſibly ſubſiſt between the warmth of the ſubſtances in queſtion, when made uſe of as cloathing, and their powers of attracting moiſture from the atmoſphere; or, in other words, between the quantities of water they contain, and their conducting powers with regard to heat; I could not find that theſe properties depended in any manner upon, or were in any way connected with, each other.

EXPERIMENTS

ON THE

RELATIVE INTENSITIES OF THE LIGHT

EMITTED BY

LUMINOUS BODIES.

[Read before the Royal Society, Feb. 6, 1794.]

BEING employed in making a number of experiments to determine, if poſſible, the moſt œconomical method of lighting up a very large workhouſe, or public manufactory, which had been erected in the ſuburbs of Munich under my direction, a method occurred to me for meaſuring the relative quantities of light emitted by lamps of different conſtructions, candles, &c. which is very ſimple, and which I have reaſon to think perfectly accurate.

Let the two burning candles, lamps, or other lights to be compared, A and B, be placed at equal heights upon two light tables, or moveable ſtands, in a darkened room; let a ſheet of clean white paper be ſpread out equally, and faſtened upon the wainſcot or ſide of the room, at the ſame height from the floor with the lights, and let the lights be placed over againſt this ſheet of paper, at the diſtance of 6 or 8 feet from it, and 6 or 8 feet from each other, in ſuch a manner, that a line drawn from the centre of the paper, perpendicular to its ſurface,

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ſhall biſect the angle formed by lines drawn from the lights to that centre; in which caſe, conſidering the ſheet of paper as a plane ſpeculum, the one light will be preciſely in the line of reflection of the other.

This may be eaſily performed, by actually placing a piece of a looking-glaſs, 6 or 8 inches ſquare, flat upon the paper, in the middle of it, and obſerving by means of it the real lines of reflection of the lights from that plane, removing it afterwards as ſoon as the lights are properly arranged.

When this is done, a ſmall cylinder of wood, about $\frac{1}{4}$ of an inch in diameter, and 6 inches long, muſt be held in a vertical poſition about 2 or 3 inches before the centre of the ſheet of paper, and in ſuch a manner, that the two ſhadows of the cylinder, correſponding to the two lights, may be diſtinctly ſeen upon the paper.

If theſe ſhadows ſhould be found to be of *unequal denſities*, which will almoſt always be the caſe, then that light whoſe correſponding ſhadow is the denſeſt, muſt be removed farther off, or the other muſt be brought nearer to the paper, till the denſities of the ſhadows appear to be *exactly equal;* or, in other words, till the denſities of the rays from the two lights are equal *at the ſurface of the paper;* when, the diſtances of the lights from the centre of the paper being meaſured, the ſquares of thoſe diſtances will be to each other as the real intenſities of the lights in queſtion at their ſources.

If, for example, the weaker light being placed at the diſtance of 4 feet from the centre of the paper,

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it ſhould be found neceſſary, in order that the ſhadows may be of the ſame denſity, to remove the ſtronger light to the diſtance of 8 feet from that centre, in that caſe, the real intenſity of the ſtronger light will be to that of the weaker as 8^2 to 4^2; or as 64 to 16; or 4 to 1; and ſo for any other diſtances.

It is well known, that if any quality do proceed from a centre, in ſtraight lines, in all directions, *like* the light emitted by a luminous body, its intenſity at any given diſtance from that centre muſt neceſſarily be as the ſquare of that diſtance inverſely; and hence it is evident that the intenſities of the lights in queſtion, at their ſources, muſt be to each other as the ſquares of their diſtances from that given point *where their rays uniting are found to be of equal denſity.* For putting x = the intenſity of the light A; and y = the intenſity of B; if P repreſent the point where the rays from A, and from B, meeting, are found to be of equal denſity or ſtrength, and if the diſtance of A from P be $= m$, and the diſtance of B from the ſame point P $= n$; then, as the intenſity of the light of A, at P, is $= \frac{x}{m^2}$, and the intenſity of the light of B, at the ſame place, is $= \frac{y}{n^2}$, and as it is $\frac{x}{m^2} = \frac{y}{n^2}$ by the ſuppoſition, it will be $x : y :: m^2 : n^2$.

That the ſhadows being of equal denſity at any given point, the intenſities of the illuminating rays muſt alſo, *of neceſſity*, be equal at that point, is evident from hence, that the total abſence of light being perfect blackneſs, and the ſhadow correſponding to one of the lights in queſtion being *deeper*, or

fainter;

fainter, according as it is *more* or *leſs enlightened*, by the other, when the ſhadows are equal, the intenſities of the illuminating rays muſt be equal likewiſe.

In removing the lights, in order to bring the ſhadows to be of the ſame denſity, care muſt be taken to recede from, or advance towards the centre of the paper in a ſtraight line, ſo that the one light may always be found exactly in the line of reflection of the other; otherwiſe the rays from the different lights falling upon the paper, and conſequently upon the ſhadows, at different angles, will render the experiment fallacious.

When the intenſity of one ſtrong light is compared with the intenſities of ſeveral ſmaller lights taken together, the ſmaller lights ſhould be placed in a line perpendicular to a line drawn to the centre of the paper, and as near to each other as poſſible; and it is likewiſe neceſſary to place them at a greater diſtance from the paper than when only ſingle lights are compared.

In all caſes, it is abſolutely neceſſary to take the greateſt care that the lights compared be properly trimmed, and that they burn clear, and equally, otherwiſe the reſults of the experiments will be extremely irregular and inconcluſive. It is aſtoniſhing what a difference there is in the quantities of light emitted by the ſame candle, when it burns with its greateſt brilliancy, and when it has grown dim for want of ſnuffing. But as this diminution of light is progreſſive, and as the eye inſenſibly conforms to the quantity of light actually preſent, it is not always taken notice of by the ſpectators;—it is nevertheleſs

very conſiderable, in fact, as will be apparent to any one who will take the trouble to make the experiment; and ſo great is the fluctuation in the quantity of light emitted by burning bodies, lamps, or candles, in all caſes, even under the moſt favourable circumſtances, that this is the ſource of the greateſt difficulties I have met with in determining the relative intenſities of lights by the method here propoſed.

Since this method of meaſuring light firſt occurred to me, I have made many improvements in the apparatus employed in it, and I have now brought the principal inſtrument to ſuch a degree of perfection, that, if I might, without being ſuſpected of affectation, I ſhould dignify it with a name, and call it a *photometer*. I have likewiſe made a conſiderable number of experiments, with a view to determining the relative quantities of light, produced by lamps and candles of different kinds, and the relative expence of lighting rooms in different ways; but before I proceed to give an account of them, it will be neceſſary to deſcribe, very particularly, the alterations I have found it expedient to make in the inſtruments employed in making them.

And, in the firſt place, the ſhadows, inſtead of being thrown upon a paper ſpread out upon the wainſcot, or ſide of the room, are now projected upon the inſide of the back part of a wooden box, $7\frac{1}{4}$ inches wide, $10\frac{1}{2}$ inches long, and $3\frac{1}{4}$ inches deep, in the clear, open in front to receive the light, and painted black on the inſide, in every part except the back, upon which the white paper is faſtened which receives the ſhadows. To the under part of

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the box is fitted a ball and ſocket, by which it is attached to a ſtand which ſupports it; and the top or lid of it is fitted with hinges, in order that the box may be laid quite open as often as it is neceſſary to alter any part of the machinery it contains. The front of the box is likewiſe furniſhed with a falling lid, or door, moveable upon hinges, by which the box is cloſed in front when it is not in actual uſe.

Finding it very inconvenient to compare two ſhadows projected by the ſame cylinder, as theſe were either too far from each other to be compared with certainty, or when they were nearer they were in part hid from the eye by the cylinder, to remedy this inconvenience, I now make uſe of two cylinders; which, being fixed perpendicularly in the bottom of the box juſt deſcribed, in a line parallel to the back part of it, diſtant from this back $2\frac{2}{10}$ inches, and from each other 3 inches, meaſuring from the centres of the cylinders; when the two lights made uſe of in the experiment are properly placed, theſe two cylinders project four ſhadows upon the white paper upon the inſide of the back part of the box, which I ſhall henceforth call *the field* of the inſtrument, two of which ſhadows are in contact preciſely in the middle of that field; and it is theſe two alone that are to be attended to. To prevent the attention being diſtracted by the preſence of unneceſſary objects, the two outſide ſhadows are made to diſappear, which is done by rendering the field of the inſtrument ſo narrow, that they fall without it, upon a blackened ſurface, upon which they are not viſible.

If the cylinders be each $\frac{4}{10}$ of an inch in diameter,

and $2\frac{2}{10}$ inches in height, (as they are in the inſtrument I have lately conſtructed) it will be quite ſufficient, if the field be $2\frac{7}{10}$ inches wide; and, as an unneceſſary height of the field is not only uſeleſs, but diſadvantageous, as a large ſurface of white paper not covered by the ſhadows produces too ſtrong a glare of light, the field ought not to be more than $\frac{3}{10}$ of an inch higher than the tops of the cylinders.

In order to be able to place the lights with facility and preciſion, a fine black line is drawn through the middle of the field, from the top to the bottom of it, and another (horizontal) line, at right angles to it, at the height of the top of the cylinders. When the tops of the ſhadows touch this laſt-mentioned line, the lights are at a *proper height*; and when further, the two ſhadows are in contact with each other in the middle of the field, the lights are then in their *proper directions*.

In my new-improved inſtrument (for I have already cauſed four to be conſtructed), the white paper which forms the field, is not faſtened immediately upon the inſide of the back of the box, but it is paſted upon a ſmall pane of very fine ground glaſs, and this glaſs, thus covered, is let down into a groove made to receive it in the back of the box. This covered glaſs is $5\frac{1}{2}$ inches long, and as wide as the box is deep, viz. $3\frac{1}{4}$ inches, but the field of the inſtrument is reduced to its proper ſize by a ſcreen of black paſteboard interpoſed before the anterior ſurface of this covered glaſs, and reſting immediately upon it. A hole in this paſteboard, in the form of an oblong ſquare, $1\frac{7}{10}$ inches wide, and two inches high,

high, determines the dimenſions, and forms the boundaries of the field. This ſcreen ſhould be large enough to cover the whole inſide of the back of the box, and it may be fixed in its place by means of grooves in the ſides of the box, into which grooves it may be made to enter. The poſition of the opening above mentioned, is determined by the height of the cylinders, the top of it being $\frac{3}{10}$ of an inch higher than the tops of the cylinders; and as the height of it is only two inches, while the height of the cylinders is $2\frac{2}{10}$ inches, it is evident that the ſhadows of the lower parts of the cylinders do not enter the field. No inconvenience ariſes from that circumſtance; on the contrary, ſeveral advantages are derived from that arrangement.

Inſtead of the ſcreen juſt deſcribed, I ſometimes make uſe of another, which differs from it only in this, that the hole in it, which determines the form and dimenſions of the field, inſtead of being quadrangular, is round, and $1\frac{6}{10}$ inches in diameter. And when this ſcreen is made uſe of, the ſhadows are increaſed in width (by means which will hereafter be deſcribed), in ſuch a manner as completely to fill the field, appearing under the form of two hemiſpheres, or rather half diſks, touching each other in a vertical line. The object I had in view in reducing the field and the ſhadows to a circular form was this; I imagined that by diminiſhing the number of objects capable of acting upon the mind, and particularly by removing all ſtraight lines and angles, and all unneceſſary varieties of lights and ſhades, the attention might be

concentrated, and fixed, in ſuch a manner as to render the ſenſe of ſight peculiarly acute, in diſtinguiſhing any difference in the ſimple objects preſented to the eye. But however plauſible this reaſoning may appear, I own the experiment did not anſwer my expectation. It is true, the apparent denſities of two equal hemiſpheres of ſhade, in contact with each other, may be compared with great facility, and when no diſcernible difference is to be perceived between them, it is more than probable that they are in fact very nearly equal; but ſtill, I have found by experience, that two equal parallelograms of ſhade, in contact with each other, may be compared with the ſame eaſe, and, I have reaſon to think, with equal certainty, and *that* even, when theſe united ſhadows are bounded on three ſides by a perfectly white ſurface, illuminated by the direct rays of two ſtrong lights; that is to ſay, when the ſcreen with the quadrangular opening or field is made uſe of.

In deſcribing the cylinders by which the ſhadows are projected, I ſaid they were fixed in the bottom of the box; but as the diameters of the ſhadows of the cylinders vary in ſome ſmall degree, in proportion as the lights are broader or narrower, and as they are brought nearer to or removed farther from the photometer, in order to be able in all caſes to bring theſe ſhadows to be of the ſame diameter, which I have found by experience to be advantageous, in order to judge with greater facility and certainty when the ſhadows are of the ſame denſity, I have rendered the cylinders move-

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able about their axes, and have added to each a vertical wing $\frac{1}{20}$ of an inch wide, $\frac{1}{16}$ of an inch thick, and of equal height with the cylinder itſelf, and firmly fixed to it from the top to the bottom. This wing commonly lies in the middle of the ſhadow of the cylinder, and as long as it remains in that ſituation it has no effect whatever; but when it is neceſſary that the diameter of one of the ſhadows ſhould be increaſed, the correſponding cylinder is moved about its axis, till the wing, juſt deſcribed, emerging out of the ſhadow, and intercepting a portion of light, brings the ſhadow projected upon the field of the inſtrument to be of the width or diameter required. In this operation, it is always neceſſary to turn the cylinder outwards, or in ſuch a manner that the augmentation of the width of the ſhadow may take place on that ſide of it which is oppoſite to the ſhadow correſponding to the other light. The neceſſity for that precaution will appear evident to any one who has a juſt idea of the inſtrument in queſtion, and of the manner of making uſe of it.

It is by means of theſe wings, attached to the cylinders, that the widths of the ſhadows are augmented, ſo as to fill the whole field of the *photometer*, when the ſcreen with the circular opening is made uſe of.

As the lower ends of the cylinders, which paſs through the holes made to receive them in the bottom of the box are about $\frac{1}{20}$ of an inch leſs in diameter than their upper parts, which caſt the ſhadows, and as they not only go quite through the bottom of the box (which is an inch thick) but

project near an inch below its inferior surface, and, lastly, as these cylinders are not firmly fixed in these holes, it is easy, by taking hold of the ends of them which project below the bottom of the box, to turn about the cylinders upon their axes, even without opening the box. I said above, that the height of the vertical wing, attached to each of the cylinders, was equal to the height of the cylinder itself:—this must be understood to mean, not the total length of the cylinder, comprehending that part of it which passes into and through the bottom of the box; but merely its height above the bottom of the box, or that part of it which projects above the bottom of the box.

As it is absolutely necessary that the cylinders should constantly remain precisely perpendicular to the bottom of the box, or parallel to each other, it will be best to construct them of brass, and instead of fixing them immediately to the bottom of the box (which being of wood may warp), to fix them to a strong, thick piece of well hammered plate brass, which plate of brass may be afterwards fastened to the bottom of the box by means of one strong screw. In this manner two of my best instruments are constructed. And, in order to secure the cylinders still more firmly in their vertical positions, they are furnished with broad flat rings, or projections, where they rest upon the brass plate; which rings are $\frac{1}{10}$ of an inch thick, and equal in diameter to the projection of the wing of the cylinder, to the bottom of which they afford a firm support. (See plate X. fig. 1.)

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Theſe cylinders are likewiſe forcibly puſhed, or rather pulled againſt the braſs plate upon which they reſt, by means of compreſſed ſpiral ſprings, placed between the under ſide of that plate and the lower ends of the cylinders.

Of whatever material the cylinders be conſtructed, and whatever be their forms or dimenſions, it is abſolutely neceſſary that they, as well as every other part of the photometer, except the field, ſhould be well painted of a deep black, dead colour. That, and that alone, will prevent the inconveniencies which would otherwiſe ariſe from reflected light, and from the preſence of too great a number of viſible objects.

In order to move the lights to and from the photometer with greater eaſe and preciſion, I provided two long and narrow, but very ſtrong and ſteady tables, in the middle of each of which there is a ſtraight groove, in which a ſliding carriage, upon which the light is placed, is drawn along by means of a cord which is faſtened to it before and behind, and which paſſing over pullies at each end of the table, goes round a cylinder, which cylinder is furniſhed with a winch, and is ſo placed, near the end of the table adjoining the photometer, that the obſerver can turn it about, without taking his eye from the field of the inſtrument. (See plate XII. fig. 3. and plate XIII. fig. 4.)

Many advantages are derived from this arrangement; as firſt, the obſerver can move the lights as he finds neceſſary, without the help of an aſſiſtant, and even without removing his eye from the ſhadows;

dows; ſecondly, each light is always preciſely in the line of direction in which it ought to be, in order that the ſhadows may be in contact in the middle of the vertical plane of the photometer; and thirdly, the ſliding motion of the lights being perfectly ſoft and gentle, that motion produces little or no effect upon the lights themſelves, either to increaſe or diminiſh their brilliancy.

Theſe tables, which are 10 inches wide and 35 inches high, and the one of them 12 feet, and the other 20 feet long, are placed at an angle of 60° from each other, and in ſuch a ſituation with reſpect to the photometer, that lines drawn through their middles in the direction of their lengths, meet in a point exactly under the middle of the vertical plane or field of the photometer, and from that point the diſtances of the lights are meaſured; the ſides of the tables being divided into Engliſh inches, and a Vernier, ſhowing tenths of inches, being fixed to each of the ſliding carriages upon which the lights are placed. (See the plates XII. and XIII.)

Theſe carriages are ſo contrived that they can be raiſed or lowered at pleaſure, which is abſolutely neceſſary in order that the lights may be always of a proper height, namely, that they may be in the ſame horizontal plane with the tops of the cylinders of the photometer.

The method of aſcertaining when the lights are at the proper height has already been deſcribed.

Inorder that the two long and narrow tables or platforms, juſt deſcribed, upon which the lights move, may remain immoveable in their proper poſitions, they

they are both firmly fixed to a very ſtrong ſtand which ſupports the photometer; and in order that the motion of the carriages which carry the lights, may be as ſoft and gentle as poſſible, they are made to ſlide upon parallel braſs wires, 9 inches aſunder, about $\frac{1}{10}$ of an inch in diameter, and well poliſhed, which are ſtretched out upon the tables from one end to the other. (See plate XII.)

The pane of glaſs covered with white paper, which, being fixed in a groove in the back of the box, conſtitutes the vertical plane upon which the ſhadows are projected, is $5\frac{1}{2}$ inches long, and $3\frac{1}{4}$ inches wide, as has already been obſerved; which is much larger than the dimenſions aſſigned above for the field; namely $1\frac{7}{10}$ inches wide, and 2 inches high. I had two objects in view in this arrangement; firſt, to render it eaſier to fix this plane in its proper poſition; and, ſecondly, to be able to augment occaſionally the dimenſions of the field, by removing entirely the black paſteboard ſcreen from before this plane, or making uſe of another with a larger aperture; which is ſometimes advantageous*.

* Since writing the above, I have made a little alteration in the form of the box which contains my photometer. The front of it, inſtead of being open, is now cloſed, and the light is admitted through two horizontal tubes, which are placed ſo as to form an angle of 60°; their axes meeting at the centre of the field of the inſtrument. (See fig. 1. plate X.) The field of the photometer is viewed through an opening made for that purpoſe in the middle of the front of the box, between the two tubes above mentioned. The plates X. XI. XII. and XIII. will ſerve to give a clearer idea of the inſtrument, in its preſent moſt improved ſtate.

Having

Having now, as I imagine, ſufficiently deſcribed all the eſſential parts of theſe inſtruments, it remains for me to give ſome account of the precautions, which, from experience, I have found it neceſſary to employ in making uſe of them.

And firſt, with reſpect to the diſtance at which lights whoſe intenſities are to be compared ſhould be placed from the field of the photometer, I have found that, when the weakeſt of the lights in queſtion is about as ſtrong as a common wax-candle, *that light* may moſt advantageouſly be placed from 30 to 36 inches from the centre of the field; and when it is weaker or ſtronger, proportionally nearer or farther off. When the lights are too near, the ſhadows will not be well defined; and when they are too far off, they will be too weak.

It will greatly facilitate the calculations neceſſary in drawing concluſions from experiments of this kind, if ſome ſteady light, of a proper degree of ſtrength for that purpoſe, be aſſumed as a ſtandard, by which all others may be compared. I have choſen for that purpoſe an ARGAND's lamp, made in London, and very well finiſhed; and though the quantity of light emitted by this, or any other kind of lamp, is very various, depending in a great meaſure upon the length to which the wick is drawn out, yet I have found by repeated trials that this lamp, once properly adjuſted, continues to emit light more equally for a conſiderable time than any other lamp, and much more ſo than any candle whatever.

At the beginning of each experiment I adjuſt this ſtandard light in the following manner. Having

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ing placed the lamp upon its carriage, at the diſtance of 100 inches from the centre of the field of the photometer, meaſuring from the centre of the circular flame of the lamp, a cylindric wax-candle, of known weight and dimenſions, and which is kept merely for that purpoſe, being lighted and trimmed, and made to burn with the greateſt poſſible degree of brilliancy, is placed over againſt it, at a certain given diſtance (33 inches), and then the wick of the lamp is drawn out, or ſhortened, as it is found neceſſary, till the ſhadows, correſponding to the lamp, and to the candle, are preciſely of the ſame denſity; this done, the proof candle is extinguiſhed, and laid by for further uſe, and the projected experiment is immediately commenced.

Here, the proof candle is, properly ſpeaking, *the ſtandard*, but the lamp is to be preferred to it for the experiments, on account of the ſuperior conſtancy, or equality of its light.

The only danger of error in this mode of proceeding, ariſes from the difficulty of procuring proof candles, which ſhall always give preciſely the ſame quantity of light, or of making the ſame candle burn with exactly the ſame brilliancy at different times; I flattered myſelf at one time that even this cauſe of error and uncertainty, however inſurmountable the difficulty appears, might be in a great meaſure removed. I conceived that if the light from the ſtandard lamp, and that of the proof candle, brought to be of the ſame intenſity at the ſurface of the vertical plane, were really ſtronger at one time than at another, the equal ſhadows of

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the cylinders would be proportionally deeper, and that by comparing, at different times, the denſity of thoſe ſhadows with a painted ſcale of ſhades, regularly graduated, any difference in the intenſity of the ſtandard light might be diſcovered and compenſated; but upon making the experiment I found, what indeed a little patient reflection would have enabled me to foreſee, that the apparent denſity of the two equal ſhadows correſponding to the lights compared with a painted ſcale of ſhades, *expoſed in the ſame light*, is ever the ſame, however the intenſity of the rays at the ſurface upon which thoſe ſhadows are projected may vary.

There is, however, another method by which I think it probable that the ſtandard lamp might be adjuſted with the requiſite degree of preciſion. It appears, from a conſiderable number of experiments, of which I ſhall hereafter give a more particular account, that the quantity of light emitted by a lamp on any given conſtruction, which burns with a clear flame, *and without ſmoke*, is in all caſes as the quantity of oil conſumed. If, therefore, the ſtandard lamp be ſo adjuſted as always to conſume a certain given quantity of oil in a given time, there is much reaſon to ſuppoſe that it may then be depended on as a juſt ſtandard of light.

In order to abridge the calculations neceſſary in theſe inquiries, it will always be advantageous to place the ſtandard lamp at the diſtance of 100 inches from the photometer, and to aſſume the intenſity of its light, at its ſource, equal to unity; in this caſe (calling this ſtandard light A,—the in-

tenſity of the light at its ſource $= x = 1$;—and the diſtance of the lamp from the field of the photometer $= m = 100$;) the intenſity of the illumination at the field of the photometer ($= \frac{x}{m^2}$*) will be expreſſed by the fraction $\frac{1}{100^2} = \frac{1}{10000}$; and the relative intenſity of any other light which is compared with it, according to the directions before given, may be found by the following proportion: calling this light B, and putting $y =$ its intenſity at its ſource, and $n =$ its diſtance from the field of the photometer, expreſſed in Engliſh inches, as it is $\frac{y}{n^2} = \frac{x}{m^2}$ as was before ſhewn; or, inſtead of $\frac{x}{m^2}$, writing its value $= \frac{1}{10000}$ it will be $\frac{y}{n^2} = \frac{1}{10000}$, and conſequently y is to 1 as n^2 is to 10000; or the intenſity of the light B at its ſource, is to the intenſity of the ſtandard light A at its ſource, as the ſquare of the diſtance of the light B from the middle of the field of the inſtrument, expreſſed in inches, is to 10000; and hence it is $y = \frac{n^2}{10000}$.

I have been the more particular in this account of the inſtruments employed in theſe inquiries,—the manner in which the experiments were conducted,—and the principles upon which the concluſions drawn from them are founded, not only becauſe the ſubject being new, the moſt particular information upon all theſe points is abſolutely neceſſary, to enable others to judge with certainty of the matter ſubmitted to their exami-

* See page 272.

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nation, but alſo, becauſe I was very deſirous of affording every information and aſſiſtance in my power to thoſe who may be diſpoſed to proſecute theſe curious and entertaining reſearches.

Hoping that this apology may be thought ſufficient to excuſe the prolixity of theſe deſcriptions, I ſhall now proceed to give a ſhort account of ſuch experiments as I have hitherto found leiſure to make with this apparatus.

My firſt attempts were to determine how far it might be poſſible to aſcertain, by direct experiments, the certainty of the aſſumed law of the diminution of the intenſity of the light emitted by luminous bodies; namely, that *the intenſity of the light is every where as the ſquares of the diſtances from the luminous body inverſely.* Theſe experiments appeared to me the more neceſſary, as it is quite evident that this law can only hold good when the light is propagated in perfectly tranſparent, or unreſiſting ſpaces, or where,—ſuffering no diminution whatever from the medium,—its intenſity is diminiſhed merely in conſequence of the divergency of the rays; and as it is more than probable that air, even in its pureſt ſtate, is far from being perfectly tranſparent.

For greater perſpicuity, I ſhall arrange all my experiments and inquiries under general heads, and ſhall begin by prefixing to thoſe which relate to the ſubject now under conſideration, the general title of

EXPERIMENTS *upon the Reſiſtance of the Air to Light.*

Experiment 1.

Two equal wax candles, well trimmed, and which were found by a previous experiment to burn with exactly the ſame degree of brightneſs, were placed *together*, on one ſide, before the photometer, and their united light was counterbalanced by the light of an ARGAND's lamp, well trimmed, and burning very equally, placed on the other ſide, over againſt them. The lamp was placed at the diſtance of 100 inches from the field of the photometer, and it was found that the two burning candles (which were placed as near together as poſſible, without their flames affecting each other by the currents of air they produced), were juſt able to counterbalance the light of the lamp at the field of the photometer, when they were placed at the diſtance of 60,8 inches from that field. One of the candles being now taken away and extinguiſhed, the other was brought nearer to the field of the inſtrument, till its light was found to be juſt able, ſingly, to counterbalance the light of the lamp; and this was found to happen when it had arrived at the diſtance of 43,4 inches.

In this experiment, as the candles burnt with equal brightneſs, it is evident that the intenſities of their united, and ſingle lights, were as 2 to 1, and in that proportion ought, according to the aſſumed theory, the ſquares of the diſtances, 60,8

and 43,4 to be; and in fact, $\overline{60,8}^2 = 3696,64$ is to $\overline{43,4}^2 = 1883,56$ as 2 is to 1 very nearly.

Again, in another experiment, (No. 2.) the distances were,

with two candles = 54 inches. Square = 2916.
with one candle = 38,6. - = 1489,96

Upon another trial (experiment No. 3)

with two candles = 54,6 inches. Square = 2981,16
with one candle = 39,7. - = 1576,09

And in the 4th experiment

with two candles = 58,4 inches. Square = 3410,56
with one candle = 42,2. - = 1780,84

And taking the mean of the results of these four experiments.

	Squares of the distances. With two candles.	With one candle.
In the experiment	No. 1, 3696,64 -	1883,56
	No. 2, 2916 -	1489,96
	No. 3, 2981,16 -	1576,09
	No. 4, 3410,56 -	1780,84
	4) 13004,36 -	4) 6736,45
	Means 3251,09 and	1682,61

which again are very nearly as 2 to 1.

With regard to these experiments, it may be observed, that were the resistance of the air to light, or the diminution of the light from the imperfect transparency of air, sensible within the limits of the inconsiderable distances at which the candles were placed from the photometer, in that case,

the diſtance of the two equal lights, united, ought to be to the diſtance of one of them ſingle, in a ratio leſs than that of the ſquare root of 2 to the ſquare root of 1. For if the intenſity of a light emitted by a luminous body, *in a ſpace void of all reſiſtance*, be diminiſhed in the proportion of the ſquares of the diſtances, it muſt of neceſſity be diminiſhed in a ſtill higher ratio when the light paſſes through a reſiſting medium, or one which is not perfectly tranſparent: and from the difference of thoſe ratios, namely, that of the ſquares of the diſtances, and that other *higher ratio* found by the experiment, the reſiſtance of the medium might be aſcertained. This I have taken much pains to do with reſpect to air, but have not as yet ſucceeded in theſe endeavours, the tranſparency of air being ſo great that the diminution which light ſuffers in paſſing through a few inches, or even through ſeveral feet of it, is not ſenſible.

Having found, upon repeated trials, that the light of a lamp, properly trimmed, is incomparably more equal than that of a candle, whoſe wick continually growing longer renders its light extremely fluctuating, I ſubſtituted lamps to candles in theſe experiments, and made ſuch other variations in the manner of conducting them, as I thought bid fair to lead to a diſcovery of the reſiſtance of the air to light, were it poſſible to render that reſiſtance ſenſible within the confined limits of my machinery.

Having provided two lamps, the one an ARGAND's lamp, which I made to burn with the greateſt poſſible brilliancy; the other a ſmall common lamp, with a ſingle, round, and very ſmall wick,

which, burning with a very clear, ſteady flame, and without any viſible ſmoke, emitted only about $\frac{1}{25}$ part as much light as the ARGAND'S lamp; theſe lamps being placed over againſt each other before the field of the photometer, their lights were found to be in equilibrium when the ſmaller being placed at the diſtance of 20 inches from the centre of that field, the greater was removed to the diſtance of 101 inches. I now concluded, that if the ſmaller light were to be removed to the diſtance of 40 inches, it would be neceſſary, in order to reſtore the equilibrium of light, or equality of the ſhadows in the field of the photometer, to remove the greater light to the diſtance of 202 inches; that is to ſay, if the diminution of the light ariſing from the imperfect tranſparency of the air ſhould not be perceptible within the limits of that diſtance. But if, on the contrary, it ſhould be found upon repeated trials, that the equilibrium was reſtored when the greater light had arrived at a diſtance *ſhort* of 202 inches, I might thence conclude, that ſuch effect might ſafely be attributed to the imperfect tranſparency of the air: for notwithſtanding that the light of the ſmaller lamp would of courſe be diminiſhed as well as that of the greater, yet as there is every reaſon to ſuppoſe that the diminution, whatever it may be, muſt ever be proportional to the diſtance through which the light paſſes in the medium, as the augmentation of the diſtance through which the light of the ſmaller lamp paſſes is no more than 20 inches, while that of the greater is made to paſs through an additional diſtance, amounting to more than 100 inches, it is

evident

evident that the diminution of the light of the greater lamp, arifing from the imperfect tranſparency of the medium, muſt be greater than the diminution of the light of the ſmaller lamp, arifing from the ſame cauſe; and, conſequently, that the effects of ſuch diminution would become apparent in the experiment were they in reality conſiderable.

The following Table will ſhew the reſults of the experiments which were made with a view to determine that fact.

Experiments.	Diſtance of the ſmaller Light.		Diſtance of the greater Light.		Second diſtance of the greater light, *computed* according to the aſſumed law of the ſquares of the diſtances.	Difference between the reſult of the experiment and the theory.
		inches.		inches.	inches.	inches.
No. 5.	First diſt.	20	First diſt.	101		
	Second diſt.	40	Second diſt.	203	202	+ 1
No. 6.	First diſt.	20	First diſt.	100,2		
	Second diſt.	40	Second diſt.	198,3	200,4	− 2,1
No. 7.	First diſt.	20	First diſt.	100,8		
	Second diſt.	40	Second diſt.	202,1	201,6	+ 0,5
No. 8.	First diſt.	20	First diſt.	101,5		
	Second diſt.	40	Second diſt.	204	203	+ 1
No. 9.	First diſt.	50	First diſt.	100		
	Second diſt.	100	Second diſt.	198	200	− 2
No. 10.	First diſt.	50	First diſt.	95,5		
	Second diſt.	100	Second diſt.	192,2	191	+ 1,2
No. 11.	First diſt.	50	First diſt.	95,1		
	Second diſt.	100	Second diſt.	191,2	190,2	+ 1
No. 12.	First diſt.	50	First diſt.	96		
	Second diſt.	100	Second diſt.	192,4	192	+ 0,4

In the four laſt experiments, inſtead of the ſmall lamp above deſcribed, a common ARGAND lamp

was made uſe of, the wick of which was only drawn out ſo far as to cauſe it to emit about $\frac{1}{4}$ part as much light as the other ARGAND's lamp, burning with its greateſt brilliancy, which was placed over againſt it.

In order that in judging of the equality of the ſhadows, my mind might be totally unbiaſſed by my expectations, or by any opinions I might previouſly have formed with reſpect to the probable iſſue of the various experiments, keeping my eye conſtantly fixed upon the field of the photometer, and cauſing the light, whoſe correſponding ſhadow was to be brought to be of equal denſity with the ſtandard, to move backwards and forwards, by means of the winch which I had conſtantly in my hand, as ſoon as the ſhadows appeared to me to be perfectly equal, I gave notice to an aſſiſtant to obſerve, and ſilently to write down, the diſtance of the lamp or candle, ſo that I did not even know what that diſtance was till the experiment was ended, and till it was too late to attempt to correct any ſuppoſed errors of my eyes, by my wiſhes, or by my expectations, had I been weak enough to have had a wiſh in a matter of this kind. I do not know that any predilection I might have had for any favourite theory, would have been able to have operated ſo ſtrongly upon my mind, and upon my ſenſes, as to have made *black* and *white* appear to me otherwiſe than as they really were; but this I know, that I was very glad to find means to avoid being *led into temptation*.

But to return to the foregoing experiments; the reſults of them, ſo far from affording means for aſcertaining the reſiſtance of the air to light, do not

even

even indicate any reſiſtance at all; on the contrary, it might almoſt be inferred from ſome of them, that the intenſity of the light emitted by a luminous body in air is diminiſhed in a ratio *leſs* than that of the ſquares of the diſtances; but as ſuch a concluſion would involve an evident abſurdity, namely, that light moving in air, its abſolute quantity, inſtead of being diminiſhed, actually goes on to *increaſe*, that concluſion can by no means be admitted.

Beſides the experiments above mentioned, I made a great number of others, ſimilar to them, and with the ſame view; but as their reſults were all nearly the ſame, I have not thought it worth while to lengthen this paper by inſerting a particular account of them. In general they all conſpired to ſhew, that the reſiſtance of the air to light was too inconſiderable to be perceptible; and that the aſſumed law of the diminution of the intenſity of the light may with ſafety be depended on.

That the tranſparency of air in its pureſt ſtate is very great, is evident from the very conſiderable diſtances at which objects, and ſuch even as are but faintly illuminated, are viſible; and I was by no means ſurpriſed that its want of tranſparency could not be rendered ſenſible in the ſmall diſtance to which my experiments were neceſſarily confined: but ſtill I think means may be found for rendering its reſiſtance to light apparent, and even of ſubjecting that reſiſtance to ſome tolerably accurate meaſure.

An accurate determination of the relative intenſity of the ſun's or moon's light, when ſeen at

different heights above the horizon, or when ſeen from the top, and from the bottom of a very high mountain, in very clear weather, would probably lead to a diſcovery of the real amount of the reſiſtance of the air to light *.

Of the Loſs of Light in its Paſſage through Plates or Panes of different Kinds of Glaſs.

In theſe experiments I proceeded in the following manner. Having provided two equal ARGAND's lamps, A and B, well trimmed, and burning with very clear bright flames, they were placed over againſt each other before the photometer, each at the diſtance of 100 inches from the field of the inſtrument, and the light of B was brought to be of the ſame intenſity as that of A, or the ſhadows were brought to be of the ſame denſity, which was done by lengthening or ſhortening the wick of the lamp B, as the occaſion required. This done,

* This method of aſcertaining the diminution of light in paſſing through the atmoſphere was propoſed, and put in practice, many years ago, by an ingenious French philoſopher, M. BOUGUER, of the Royal Academy of Sciences. See Traitè d'Optique ſur la Gradation de la Lumiere, Ouvrage poſthume de M. Bouguer, de l'Academie Royale des Sciences, &c. Publiſhed at Paris by the Abbè DE LA CAILLE in the year 1760.

When this paper was written, I had not ſeen that moſt ingenious and learned diſſertation. It did not come into my hands till a few months ago, (in November 1801) when, being at Paris, my worthy and reſpectable friend the SENATOR LAPLACE procured it for me.

and

and the two lamps now burning with precisely the same degree of brilliancy, a pane of fine, clear, transparent, well polished glass, such as is commonly made use of in the construction of looking-glasses, six inches square, placed vertically upon a stand, in a small frame, was interposed before the lamp B, at the distance of about four feet from it, and in such a position that the light emitted by it, was obliged to go perpendicularly through the middle of the pane, in order to arrive at the field of the photometer. The consequence of this was, that the light of the lamp B being diminished and weakened in its passage through the glass, the illuminations of the shadows in the field of the photometer were no longer equal, the shadow corresponding to the lamp A being now less enlightened by the light of the lamp B, than the shadow corresponding to the lamp B was enlightened by the undiminished light of the lamp A.

To determine precisely the exact amount of this diminution of the light of the lamp B (which was the main object of the experiment), nothing more was necessary than to bring this lamp nearer to the field of the photometer, till its light passing through the glass should be in equilibrium with the direct light of the lamp A; or, in other words, till the equality of the shadows should be restored; and this I found actually happened when the lamp B, from 100 inches, was brought to the distance of 90,2 inches from the field of the photometer.

Now as it has already been shewn that the intensities of the lights are as the squares of their

distances

diſtances from the field of the photometer, the illuminations being equal at that field, it is evident that the light of the lamp B was diminiſhed, in this experiment, in its paſſage through the pane of glaſs, in the ratio of 100^2 to $90,2^2$, or as 1 to ,8136; ſo that no more than ,8136 parts of the light which impinged againſt the glaſs found its way through it; the other ,1864 parts being diſperſed and loſt.

To aſſure myſelf that the lamps ſtill continued to emit the ſame relative quantities of light as at the beginning of the experiment, I now removed the pane of glaſs, and found that the equality of the ſhadows was again reſtored, when the lamp B arrived at its former ſtation, 100 inches from the field of the photometer.

This experiment I repeated no leſs than 10 times, and found the loſs of light in its paſſage through this pane of glaſs, taking a mean of all the experiments, to be ,1973 parts of the whole quantity that impinged againſt it; the variations in the reſults of the various experiments being from ,1720 to ,2108.

In four experiments, with another pane of the ſame kind of glaſs, the loſs of light was ,1836; ,1732; ,2056; and ,1853; mean ,1869.

When the two panes of this glaſs were placed before the lamp B, at the ſame time, but without touching each other, and the light made to paſs through them both, the loſs of light, in four different experiments, was ,3089; ,3259; ,3209; and ,3180; mean ,3184.

With

With another pane of glaſs of the ſame kind, but a little thinner, the mean loſs of light, in four experiments, was ,1813.

With a very thin, clean pane, of clear, white, or colourleſs window-glaſs, not ground, the loſs of light, in four experiments, was ,1324; ,1218; ,1213; and ,1297; mean ,1263. When the experiment was made with this ſame pane of glaſs, a very little dirty, the loſs of light was more than doubled.

Might not this apparatus be very uſefully employed by the optician, to determine the degree of tranſparency of the glaſs he employs, and direct his choice in the proviſion of that important article in his trade?

In making theſe experiments, a great deal of the trouble may well be ſpared, for there is no uſe whatever in bringing the two lamps A and B to burn with the ſame degree of brilliancy; all that is neceſſary being to bring the ſhadows to be of the ſame denſity, with the glaſs, and without it, noting the diſtance of the lamp B in each caſe (the lamp A remaining immoveable in its place); for the relative quantity of light loſt will ever be accurately ſhewn by the ratio of the ſquares of thoſe diſtances, whatever be the relative brilliancy with which the two lamps burn. The experiment is more ſtriking, and the conſequences drawn from it rather more obvious, when the lamps are made to burn with equal flames; otherwiſe that equality is of no real advantage.

Of

Of the Loſs of Light in its Reflection from the Surface of a plane Glaſs Mirror.

In theſe experiments the method of proceeding was much the ſame as in thoſe juſt mentioned. The lamps A and B, burning with clear, bright, and ſteady flames, were placed before the field of the photometer, and one of them was moved backwards and forwards till the illuminations of the ſhadows in the field of the inſtrument were found to be preciſely equal. The diſtance of the lamp B being then noted, this lamp was removed, and a mirror being put in its place, but nearer the field of the photometer, the lamp was ſo placed that its rays, ſtriking the centre of the mirror, were reflected againſt the field of the photometer, where, by bringing the lamp nearer to, or removing it farther from the mirror, the illumination of the field by thoſe reflected rays was now brought to be in equilibrium with the illumination of the ſtandard lamp, and then the diſtance of the lamp from the centre of the mirror, and the diſtance from thence to the centre of the field, were carefully meaſured and noted. Theſe two diſtances added together, was the real diſtance through which the rays paſſed in order to arrive at the field of the photometer.

Now as there is always a loſs of light in reflection, it is evident that the reflected rays muſt come to the field of the photometer weakened, and that in order to illuminate this field by theſe reflected rays as ſtrongly as it was illuminated by the direct rays of the ſame lamp, the lamp muſt be brought

nearer

nearer to the field. It is likewiſe evident, from what has already been ſaid, that the ratio of the ſquares of thoſe diſtances of the lamp when its rays paſs on directly, and when they arrive after having been reflected, are found to illuminate equally the field of the photometer, will be an accurate meaſure of the loſs of the light in reflection.

The following Table will ſhew the reſults of five experiments with a ſmall, but moſt excellent glaſs mirror, made by RAMSDEN. This mirror, which makes part of an optical inſtrument I cauſed to be conſtructed in London about twelve years ago, is 7 inches long, and $5\frac{1}{2}$ inches wide, and I ſuppoſe is as perfect as ever glaſs mirror was of that ſize.

To facilitate the compariſon of the reſults of the experiments, the lamp B, at the beginning of each experiment (when the intenſity of its direct rays was compared with the intenſity of the ſtandard lamp), was placed at the diſtance of 100 inches, the ſtandard lamp being occaſionally moved, in order to produce an equality of the ſhadows.

Experiments.	The angle of incidence.	Diſtance of the centre of the mirror from the centre of the field.	Diſtance of the lamp from the centre of the mirror.	Real diſtance of the lamp, or length of the reflected rays.	Light loſt in the reflection.
		inches.	inches.	inches.	parts.
1	60°	40	40,8	80,8	,3472
2	85°	—	41,	81,	,3439
3	45°	—	41,5	81,5	,3358
4	60°	—	39,5	79,5	,3680
5	70°	—	40,5	80,5	,3520

The

The mean of theſe five experiments gives for the loſs of light ,3494; and from hence it appears, that more than $\frac{1}{3}$ part of the light which falls upon the beſt glaſs mirror that can be conſtructed is loſt in reflection.

The loſs with mirrors of indifferent quality, is ſtill more conſiderable. With a very bad common looking-glaſs the loſs, in one experiment, appeared to be ,4816 parts; and with another looking-glaſs it was ,4548 parts in one experiment, and ,4430 in another. I ſhould certainly have made an experiment to determine the loſs of light in its reflection from the ſurface of a plane metallic mirror, but I had no ſuch mirror at hand.

The difference of the angles of incidence at the ſurface of the mirror, within the limits mentioned, namely, from 45° to 85°, did not appear to affect, in any ſenſible degree, the reſults of the experiments. I alſo found upon trial, that the effect produced by the difference of the angles at which light impinges againſt a ſheet of tranſparent glaſs through which it paſſes, is, within the limits of 40° or 50° from the perpendicular, but very trifling.

Of the relative Quantities of Oil conſumed, and of Light emitted, by an ARGAND'S *Lamp, and by a Lamp on the common Conſtruction, with a Riband Wick.*

The brilliancy of the ARGAND'S lamp is not only unrivalled, but the invention is, in the higheſt degree,

degree, ingenious, and the inſtrument uſeful for many purpoſes; but ſtill, to judge of its real merits, as an illuminator, it was neceſſary to know whether it gives more light than another lamp *in proportion to the oil conſumed.* This point I determined in the following manner.

Having placed an ARGAND's lamp, well trimmed, and burning with its greateſt brilliancy, before my photometer, and over againſt it a very excellent common lamp, with a riband wick, about an inch wide, and which burnt with a clear, bright flame, without the leaſt appearance of ſmoke, I found the intenſities of the light emitted by the two lamps to be to each other as 17956 to 9063; the denſities of the ſhadows being equal when the ARGAND's being placed at the diſtance of 134 inches, the common lamp was placed at the diſtance of 95,2 inches, from the field of the photometer.

Both lamps having been very exactly weighed when they were lighted, they were now (without being removed from their places before the photometer,) cauſed to burn with the ſame brilliancy juſt 30 minutes; when they were extinguiſhed, and weighed again, and were found to have conſumed of oil, the ARGAND's lamp $\frac{253}{8192}$, and the common lamp $\frac{163}{8192}$ of a Bavarian pound.

Now as the quantity of light produced by the ARGAND's lamp, in this experiment, is to the quantity produced by the common lamp, as 17956 to 9063, or as 187 to 100; while the quantity of oil conſumed by the former is to that conſumed by the latter only in the ratio of 253 to 163, or as 155 to

100,

100, it is evident that the quantity of light produced by the combuſtion of a given quantity of oil in an ARGAND's lamp is greater than that produced by burning the ſame quantity in a common lamp, in the ratio of 187 to 155, or as 100 to 85.

The ſaving, therefore, of oil which ariſes from making uſe of an ARGAND's lamp, inſtead of a common lamp, in the production of light, is evident; and it appears from this experiment that that ſaving cannot amount to leſs than 15 *per cent.* How far the advantage of this ſaving may, under certain circumſtances, be counterbalanced by inconveniences that may attend the making uſe of this improved lamp, I will not pretend to determine.

Of the relative Quantities of Light emitted by an ARGAND's *Lamp, and by a common Wax Candle.*

I have made a conſiderable number of experiments to determine this point, and the general reſult of them is, that a common ARGAND's lamp, burning with its uſual brightneſs, gives about as much light as *nine good wax candles*; but the ſizes and qualities of candles are ſo various, and the light produced by the ſame candle ſo fluctuating, that it is very difficult to aſcertain, with any kind of preciſion, what a common wax candle is, or how much light it ought to give. I once found that my ARGAND's lamp, when it was burning with its greateſt brilliancy, gave twelve times as much light as a good wax candle $\frac{3}{4}$ of an inch in diameter, but never more.

Of

Of the Fluctuations of the Light emitted by Candles.

To determine to what the ordinary variations in the quantity of light emitted by a common wax candle might amount, I took ſuch a candle, and, lighting it, placed it before the photometer, and over againſt it an ARGAND's lamp, which was burning with a very ſteady flame; and meaſuring the intenſity of the light emitted by the candle from time to time, during an hour, the candle being occaſionally ſnuffed when it appeared to ſtand in need of it, its light was found to vary from 100 to about 60. The light of a wax candle of an inferior quality was ſtill more unequal, but even this was but trifling compared to the inequalities of the light of a tallow candle.

An ordinary tallow candle, of rather an inferior quality, having been juſt ſnuffed, and burning with its greateſt brilliancy, its light was as 100; in eleven minutes it was but 39; after eight minutes more had elapſed, its light was reduced to 23; and in ten minutes more, or twenty-nine minutes after it had been laſt ſnuffed, its light was reduced to 16. Upon being again ſnuffed, it recovered its original brilliancy, 100.

Of the relative Quantities of Bees Wax, Tallow, Olive Oil, Rape Oil, and Linſeed Oil, conſumed in the Production of Light.

In order to aſcertain the relative quantities of bees wax, and of olive oil, conſumed in the produc-

tion of light, I proceeded in the following manner. Having provided an end of a wax candle of the beſt quality, ,68 of an inch in diameter, and about four inches in length, and a lamp with five ſmall wicks, which I had found upon trial to give the ſame quantity of light as the candle, I weighed very exactly the candle, and the lamp filled with oil, and then placing them at equal diſtances (40 inches) before the field of the photometer, I lighted them both at the ſame time; and after having cauſed them to burn with preciſely the ſame degree of brightneſs *juſt one complete hour*, I extinguiſhed them both, and weighing them a ſecond time, I found that 100 parts of wax, and 129 parts of oil, had been conſumed.

Hence it appears, that the conſumption of *bees wax* is to the conſumption of *olive oil*, in the production of the *ſame given quantity of light*, as 100 is to 129.

In this experiment no circumſtance was neglected that could tend to render the reſult of it concluſive. Care was taken to ſnuff the candle very often, with a pair of ſharp ſciſſars, in order to make it burn conſtantly with the ſame degree of brilliancy; and the light of the lamp was, during the whole time, kept in the moſt exact equilibrium with the light of the candle; which was eaſily done by occaſionally drawing out, a little more or leſs, one or more of its five equal wicks. Theſe wicks, which were placed in a right line perpendicular to a line drawn from the middle wick to the middle of the field of the photometer, were about $\frac{1}{10}$ of an inch in diame-

ter each, and $\frac{1}{4}$ of an inch from each other, and when they were lighted, their flames united into one broad, thin, and very clear, white flame, without the leaſt appearance of ſmoke.

In order to aſcertain the relative conſumption of olive oil and rape oil, in the production of light, two lamps, like that juſt deſcribed, were made uſe of; and the experiment being made with all poſſible care, the conſumption of *olive oil* appeared to be to that of *rape oil*, in the production of the ſame quantity of light, as 129 is to 125.

The experiment being afterwards repeated with *olive oil* and very pure *linſeed oil*, the conſumption of olive oil appeared to be to that of the linſeed oil as 129 to 120.

The experiment being twice made with *olive oil*, and with *a tallow candle*; once when the candle, by being often ſnuffed, was made to burn conſtantly with the greateſt poſſible brilliancy, and once when it was ſuffered to burn the whole time with a very dim light, owing to the want of ſnuffing, the reſults of theſe experiments were very remarkable.

When the candle burnt with a clear, bright flame, the conſumption of the olive oil was to the conſumption of the tallow as 129 is to 101; but when the candle burnt with a dim light, the conſumption of the olive oil was to the conſumption of the tallow as 129 is to 229. So that it appeared from this laſt experiment, that the tallow, inſtead of being nearly as productive of light in its combuſtion as bees wax, as it appeared to be when the candle was kept conſtantly well-ſnuffed, was now, when

the candle was ſuffered to burn with a dim light, by far leſs ſo than oil.

But this is not all; what is ſtill more extraordinary is, that the very ſame candle, burning with a long wick, and a dim light, actually conſumed *more tallow* than when, being properly ſnuffed, it burnt with a clear, bright flame, and gave near *three times as much light!*

To be enabled to judge of the relative quantities of light actually produced by the candle in the two experiments, it will ſuffice to know, that in order to counterbalance this light at the field of the photometer, it required, in the former experiment, the conſumption of 141 parts, but in the latter only the conſumption of 64 parts of olive oil. But in the former experiment 110 parts, and in the latter 114 parts of tallow were actually found to be conſumed. Theſe parts were 8192ths of a Bavarian pound.

From the reſults of all the foregoing experiments it appears, that the relative expence of the undermentioned inflammable ſubſtances, in the production of any given quantity of light, is as follows:

		Equal parts in weight.
Bees wax.	A good wax candle, kept well ſnuffed, and burning with a clear, bright flame, -	100
Tallow.	A good tallow candle, kept well ſnuffed, and burning with a bright flame, -	101
	The ſame tallow candle, burning very dim for want of ſnuffing, - -	229

Olive

		Equal parts in weight.
Olive oil.	Burnt in an ARGAND's lamp,	110
	The ſame burnt in a common lamp, with a clear, bright flame, without ſmoke, -	129
Rape oil.	Burnt in the ſame manner, -	125
Linſeed oil.	Likewiſe burnt in the ſame manner, - -	120

I ſhould have been very glad to have made the experiment with whale oil, but there was none to be had in the country I inhabited at that time, (Bavaria.)

With the foregoing table, and the prices current of the therein-mentioned articles, the relative *prices of light* produced by thoſe different materials may very readily be computed.

The light of a wax candle, for inſtance, coſts juſt *nine times* more at Munich, than the ſame quantity of light produced by burning rape oil in an ARGAND's lamp.

Of the Tranſparency of Flame.

To aſcertain the tranſparency of flame, or the meaſure of the reſiſtance it oppoſes to the paſſage of foreign or extraneous light through it, I placed before the photometer, over againſt the ſtandard lamp, two burning wax candles, well trimmed; and putting them near together, ſometimes by the ſides of each other, and ſometimes in a ſtraight line behind each other, I found that when their diſtances from the field of the photometer were the ſame,

the intenſity of the illumination was to all appearance the ſame, whether the light of the one was made to paſs through the flame of the other, or not. And the ſame held good, with very little variation, when three, and even when four candles were made uſe of in the experiment, inſtead of two.

I even cauſed a lamp to be conſtructed with nine round wicks, placed in an horizontal line, and juſt ſo far aſunder as to prevent their flames uniting, and no farther. And I found, upon repeating the experiment with this lamp, that the reſult was much the ſame as with the candles; the intenſity of the illumination at the field of the photometer being very nearly the ſame, whether theſe nine lights were placed ſo as to cover, and paſs through each other, or not.

But I afterwards found means to demonſtrate the very great tranſparency of flame by a ſtill more ſimple experiment. Suſpecting that the only reaſon why bodies are not viſible through a ſheet of vivid flame is, that the light of the flame affects the eye in ſuch a manner as to render it inſenſible to the weaker light emitted by, or reflected from the objects placed behind it, I conceived that a very ſtrong light would not only be viſible through a weak flame, but alſo (as all tranſparent bodies are inviſible) that it might perhaps cauſe the flame totally to diſappear: to determine that fact, I took a lighted candle, at mid-day, the ſun ſhining moderately bright, and holding it up between my eye and the ſun, I found the flame of the candle to

diſappear

diſappear entirely. It was not even neceſſary, in order to cauſe the flame to become inviſible, to bring it to be directly between the eye and the body of the ſun; it was ſufficient for that purpoſe to bring it into the neighbourhood of the ſun, where the light was very ſtrong: even in a ſituation in which the light was not ſo ſtrong as to dazzle the eye ſo much as to prevent its ſeeing very diſtinctly the body of the candle and the wick, not the leaſt appearance of flame was diſcernible, though the candle actually burnt the whole time very vigorouſly.

DESCRIPTION OF THE PLATES.

Plate X. fig. 1. This repreſents a plan, or rather the outlines of a birds-eye view of the photometer, upon its ſtand, together with the ends adjoining to the ſtand of the long and narrow tables on which the carriages run which ſupport the lights, *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *k*, is the plan of the photometer properly ſo called, which is a box of wood, painted black within and without, with two projecting, horizontal, quadrangular tubes, *e*, *f*, *g*, *h* and *i*, *k*, *a*, *b*, through which the light is admitted. The part of the figure which is bounded by the three ſtraight lines *g*, *h*,—*h*, *i*,—and *i*, *k*, and the curved line *k*, *g*, is merely a projection of the board which forms the bottom of the box; it is of no real uſe, ſerving only to give a more elegant form to the inſtrument.

Dotted lines drawn through the axes of the two horizontal tubes abovementioned, meet at the ſurface of a vertical plane conſiſting of a piece of ſheet glaſs, covered with white paper, which plane conſtitutes the field of the inſtrument, on which the ſhadows are projected.

Two ſmall circles through which thoſe dotted lines paſs, repreſent the ground plans of the two cylinders of braſs, painted black, by which the ſhadows are thrown on the field of the photometer. On one ſide of each of theſe cylinders there is a projecting wing, a plan of which is repreſented in the figure.

Each of the ſmall circles which repreſent the plans, or horizontal ſections of the cylinders, is ſurrounded by another circle, about three times as large, which repreſents a flat horizontal circular plate of braſs, about $\frac{1}{10}$ of an inch thick, on which the cylinder ſtands, and to which it is firmly faſtened by ſolder. Theſe circular plates are placed on an oblong horizontal plate of braſs, through which the cylinders, which are continued below the circular plates, paſs in two holes in the oblong plate which are made to receive them.

To the lower ends of each of the cylinders, which, paſſing through the bottom of the wooden box which conſtitutes the body of the photometer, project downward about an inch below it, is fixed a thumb-piece, or handle, (viſible in the fig. 2. plate XI.)

Theſe thumb-pieces ſerve for turning the cylinders about their axes; which is done occaſionally in order to bring the ſhadows of the two cylinders,

which

which are thrown on the field of the inſtrument, to be of the ſame width. The manner in which this is effected will be evident if we conſider that as long as the vertical wing which is annexed to each of the cylinders remains in the ſhadow of its cylinder, it cannot add to the width of the ſhadow caſt on the vertical plane which conſtitutes the field of the photometer, but as ſoon as, by turning the cylinder about its axis, that wing is made to immerge from the ſhadow of the cylinder, on one ſide, the width of the ſhadow on the field of the inſtrument will be increaſed. By theſe means the widths of the two ſhadows, which are compared, may at any time be made equal; and they ſhould be ſo, in order that their intenſities may be compared with greater facility and accuracy. As often as the two lights, which are the ſubjects of an experiment, are placed at different diſtances from the field of the photometer, the ſhadows of the two equal cylinders, unaſſiſted by their projecting wings, will of courſe be of unequal widths. To bring their widths to be equal was the ſole object of the contrivance we have been deſcribing.

L, *m*, *n*, (fig. 1. plate X.) are the three ſtrong feet which ſupport the photometer, and alſo a round table on which one end of each of the long narrow tables reſts, that ſupport the ſliding carriages which carry the lights. In each of theſe feet there is a ſcrew, (repreſented more diſtinctly in the figure 2, plate XI.) by means of which the ſtand, or pillar which ſupports the photometer, may be brought into a poſition exactly vertical.

A ground plan of a part of one of the long and narrow tables, (that on the right hand) is repreſented in this figure; and a part alſo of one of the carriages which carry the lights is ſeen, at *o*, *p*, *q*, *r*. The top of the pulley is alſo ſeen, and the line which, paſſing over it, draws the carriage on which the light ſtands. The place occupied by one end (that next to the photometer) of the other long table, is repreſented by the dotted lines *t*, *u*, *w*, *x*. The place of the ſtrong pin which, paſſing through an hole made to receive it, near the end of the table, is repreſented (in a ground plan) at *s*. Theſe pins are ſhewn very diſtinctly at *s*, *s*, in the figure 2, plate XI.

1, 2, 3, figures 1 and 2, are three ſtrong braces which aſſiſt in ſupporting the pillar, on the top of which the photometer is placed.

T in the figures 1 and 2, is a ſtrong circular table, on which one end of each of the long narrow tables is ſupported. This circular table, through the centre of which the pillar of the photometer paſſes, is ſupported on a ſtrong flanch or ſhoulder in the pillar which is made for it to reſt upon.

The box of the photometer is fixed to its ſtand, or pillar, by means of a ball and ſocket. In the figure 2. this box is repreſented ſhut up, by three ſliding wooden doors, *a*, *b*, and *c*. Through the door-way on the left at *a*, and through that on the right at *b*, light is admitted into the photometer,—and that in the middle, at *c*, is opened in order to obſerve the ſhadows caſt on the field of the inſtrument.

The

The places occupied by theſe three ſliding-doors in the ground plan of the photometer, (ſee fig. 1.) are as follows. The firſt (*a*) fills the opening from *a* to *k*. The ſecond (*b*) that from *f* to *g*; and the third (*c*) that from *i* to *h*.

Fig. 3, plate XII. This figure repreſents a plan, or rather the outlines of a birds-eye view of the whole of the apparatus, drawn to a ſmall ſcale.

a is the box of the photometer, which is repreſented as being cloſed above with its lid, or wooden cover.

b and *c*, are the two ſliding carriages on which the lights are placed, which are the ſubjects of the experiments. There is a moveable ſtage or platform belonging to each of theſe carriages, which, by means which will preſently be deſcribed, can be placed higher or lower. It is upon theſe platforms, and not on the bottoms of the carriages, that the lights are placed; and as they are moveable, upwards and downwards, the lights to be compared can eaſily be placed exactly at the ſame height, which is always neceſſary. Each of the pieces of board which form theſe platforms, has three holes through it, in which three cylindrical pillars paſs, which ſtand on the bottom of the carriage, and are firmly fixed in it. The platform is attached to theſe three pillars, at any height above the bottom of the carriage, by means of ſmall horizontal ſcrews, which can be made to preſs againſt the pillars. Theſe ſcrews are fixed in large hollow knobs of wood which are fixed to the platform, juſt over the

holes,

holes, in ſuch a manner that each pillar paſſes through the axis of one of theſe knobs.

One of theſe knobs, together with the end of the ſcrew by which it is faſtened to the pillar, is repreſented in the birds-eye view of the carriage *b*, and another in that *c*, figure 3. The reaſon why the other two knobs, belonging to each of theſe carriages, are not ſeen, is this: They are hid by a flat narrow piece of wood (repreſented in the figure), which, paſſing from the top of one of the two front pillars of the carriage to the other, ſerves to make thoſe pillars more ſteady. A front view of the three knobs belonging to each of the carriages, may be ſeen in the next figure.

d and *e*, are the winches by means of which the ſliding carriages, *b* and *c*, are occaſionally brought nearer to, and carried farther from the field of the photometer. The ſtrong wires ſtretched along upon each ſide of each of the long tables on which the carriages ſlide, are repreſented in this figure; as alſo the cord ſtretched along the middle of each table, and paſſing over pullies at each end of it, and round the cylinder of the winch, which ſerves for drawing the carriage backwards and forwards.

The two ends of this cord are united under the table, forming of the whole a kind of band, which is kept at a proper degree of tenſion by a weight under the table which is fixed to a pully. This weight is ſeen in the next figure, (plate XIII.) ſuſpended by the cord under one of the tables. T is the circular table, which is repreſented on a much larger ſcale in the figures 1 and 2.

In

In this figure (3) and in the next, the brackets are ſeen which ſupport the ends of the long tables which are fartheſt from the ſtand of the photometer. Each of theſe brackets is furniſhed with two ſcrews, diſtinctly repreſented in the figure, which ſerve for ſetting the table on a true horizontal level.

Plate XIII. fig. 4. This figure is an elevation of the whole of the machinery, ſeen in the direction of the length of one of the long tables. The two tables are ſuppoſed to be ſo placed as to form an angle of 60°. in which ſituation they are alſo repreſented in the laſt figure. As in this figure (4) one end of one of the long tables is repreſented as ſtanding immediately before the ſtand of the photometer, the ſliding carriage belonging to that table obſtructs the view of the upper part of the ſtand, and of the box of the photometer, and renders the appearance of the machinery in that part of the plate rather confuſed; but, by a careful examination, the different parts of it may be diſtinguiſhed.

The platforms on which the lights are placed, are repreſented as being both fixed at the ſame horizontal level; and all the ſix hollow knobs of wood are diſtinctly ſeen, by which they are faſtened to the ſlender pillars which ſupport them. The lights themſelves are not repreſented in any of theſe figures. The handles of the winches, by means of which the lights are moved backwards or forwards, by an obſerver who is ſetting before the photometer, and looking at the ſhadows, are both repreſented in this figure.

Care

Care muſt always be taken, in making the experiments, to place the two lights and the centre of the field of the photometer in the ſame plane.

The frames of the long tables are conſtructed of ſtrong deal boards, placed edge-ways, and the two long boards which form the two ſides of each table are made narrower at that end of them which is next to the ſtand of the photometer, in the manner repreſented in the figure 4.

This is done to give more room to the obſerver, when he is ſitting before the inſtrument to obſerve the ſhadows. The winches are ſo placed, that he can conveniently keep one of them in each hand, and turn them about while his eye remains fixed on the field of the inſtrument.

In order that the weight *w*, by which the cord is kept properly ſtretched, may be forced to remain in its proper place, the cord is made to paſs over two additional pullies at *a* and *b*. The manner in which theſe pullies act will be evident from a bare inſpection of the figure.

The upper edges of the two long boards which conſtitute the inſides of the frames of the two long tables, are divided in feet and inches, which greatly facilitates the aſcertaining of the diſtances of the lights from the field of the photometer.

At the ends of the long tables the pins are ſeen, by means of which the wires are ſtretched, on which the carriages of the lights ſlide.

AN ACCOUNT

OF SOME

EXPERIMENTS

ON

COLOURED SHADOWS.

[Read before the Royal Society, February 20, 1794.]

WHILE I was employed in the profecution of my experiments on the intenſities of light, I was ſtruck with a very beautiful, and what I then conſidered as a new appearance.—Deſirous of comparing the intenſity of the light of a clear ſky, by day, with that of a common wax candle, I darkened my room, and letting the daylight from the north (coming through a hole near the top of the window-ſhutter), fall at an angle of about 70° upon a ſheet of very fine white paper, I placed a burning wax candle in ſuch a poſition that its rays fell upon the ſame paper, and, as nearly as I could gueſs, in the line of reflection of the rays of daylight from without; when, interpoſing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the diſtance of about two inches from its ſurface, I was much ſurprized to find that the two ſhadows projected by the cylinder upon the paper, inſtead of being merely ſhades, *without colour*, as I expected to find them,

the

the one of them,—that which, correſponding with the beam of daylight, was illuminated by the candle,—was *yellow*; while the other, correſponding to the light of the candle,—and conſequently illuminated by the light of the heavens,—was of the moſt beautiful *blue* that it is poſſible to imagine. This appearance, which was not only unexpected, but was really in itſelf in the higheſt degree ſtriking and beautiful, I found, upon repeated trials, and after varying the experiment in every way I could think of, to be ſo perfectly permanent, that it is abſolutely impoſſible to produce *two ſhadows* at the ſame time from the ſame body, the one anſwering to a beam of daylight, and the other to the light of a candle or lamp, without theſe ſhadows being coloured, the one *yellow* and the other *blue*.

The experiment may very eaſily be made, at any time by day, and almoſt in any place, and even by a perſon not in the leaſt degree verſed in experimental reſearches. Nothing more is neceſſary for that purpoſe, than to take a burning candle into a darkened room, in the day time, and open one of the window-ſhutters a little, about half or three quarters of an inch for inſtance; when, the candle being placed upon a table, or ſtand, or given to an aſſiſtant to hold, in ſuch a ſituation that the rays from the candle may meet thoſe of daylight from without, at an angle of about 40°, at the ſurface of a ſheet of white paper, held in a proper poſition to receive them, any ſolid opaque body, a cylinder, or even a finger, held before the paper, at the diſtance of two or three inches, will project two

ſhadows

ſhadows upon the paper, the one blue, and the other yellow.

If the candle be brought nearer to the paper, the blue ſhadow will become of a deeper hue, and the yellow ſhadow will gradually grow fainter; but if it be removed farther off, the yellow ſhadow will become of a deeper colour, and the blue ſhadow will become fainter; and the candle remaining ſtationary in the ſame place, the ſame varieties in the ſtrength of the tints of the coloured ſhadows may be produced merely by opening the window-ſhutter a little more or leſs, and rendering the illumination of the paper by the light from without ſtronger or weaker. By either of theſe means, the coloured ſhadows may be made to paſs through all the gradations of ſhade, from the deepeſt to the lighteſt, and *vice verſa*; and it is not a little amuſing to ſee ſhadows, thus glowing with all the brilliancy of the pureſt and moſt intenſe priſmatic colours, then paſſing ſuddenly through all the varieties of ſhade,—preſerving in all the moſt perfect purity of tint,—growing ſtronger, and fainter,—and vaniſhing and returning at command.

With reſpect to the cauſes of the colours of theſe ſhadows, there is no doubt but they ariſe from the different qualities of the light by which they are illuminated; but how they are produced, does not appear to me ſo evident *. That the ſhadow corre-

ſponding

* I ought to inform the reader, that when the above was written, I had not the ſmalleſt recollection of what, many years before,

ſponding to the beam of daylight, which is illuminated by the yellow light of a candle, ſhould be of a yellowiſh hue, is not ſurpriſing; but why is the ſhadow correſponding to the light of the candle,

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I had read concerning coloured ſhadows, in Prieſtley's Hiſtory of Optics. It may perhaps be thought, (by others, as well as by myſelf,) that it was a fortunate circumſtance that I had forgotten what I had read; for it left my mind in perfect freedom to purſue, in my own way, the inveſtigation of the cauſes of the phænomena which preſented themſelves to my obſervation, without my being biaſed by the opinions of others, who, before me, had attempted to explain them. Had I recollected what others had done, I ſhould not, moſt probably, have given myſelf the trouble of engaging in the proſecution of theſe enquiries.

But, although, *at the time when this paper was written*, I had really no remembrance whatever, of what had been written and publiſhed before, on this ſubject; yet, ſoon after the paper was finiſhed, and ſome time before it was ſent to England to be laid before the Royal Society, I was, by an accidental circumſtance, made to recollect what I had ſo entirely forgotten.—Shall I confeſs what the motives were which induced me to expoſe myſelf to the danger of being thought *ignorant*, or ſomething worſe, by ſuffering my paper to go out of my hands without alteration? ——When the glow of the ſudden bluſh which I felt on diſcovering my danger had paſſed off, and I had taken time to reflect coolly on all the circumſtances of the caſe, I concluded that it might be *uſeful* to permit my paper to go forth into the world in its original ſtate. I conceived that it would ſhew, in a very ſtriking manner—if not the advantages which ſometimes reſult from forgetting what we have read—at leaſt the very great importance of preſerving the mind totally unbiaſed by the ſpeculative opinions of others when we are in ſearch of truth.

An ardent lover of Science will not heſitate to expoſe himſelf to *perſonal danger*, when he perceives that by ſo doing he has a chance of promoting uſeful inveſtigation.

and

and which is illuminated by no other light than the apparently white light of the heavens, *blue?* I at firſt thought that it might ariſe from the blueneſs of the ſky; but finding that the broad daylight, reflected from the roof of a neighbouring houſe covered with the whiteſt new-fallen ſnow, produced the ſame blue colour, and if poſſible of a ſtill more beautiful tint, I was obliged to abandon that opinion.

To aſcertain with ſome degree of preciſion the *real colour* of the light emitted by a candle, I placed a lighted wax candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new-fallen ſnow, and the heavens were overſpread with white clouds; when, the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided *yellow colour*, not even approaching to whiteneſs.

The flame of an ARGAND's lamp, expoſed at the ſame time, in the open air, appeared to be of the ſame yellow hue. But the moſt ſtriking manner of ſhewing the yellow hue of the light emitted by lamps and candles, is, by expoſing them in the direct rays of a bright meridian ſun. In that ſituation, the flame of an ARGAND's lamp, burning with its greateſt brilliancy, appears in the form of a dead yellow ſemi-tranſparent ſmoke. How tranſcendently pure, and inconceivably bright, the rays of the ſun are, when compared to the light of any of our artificial illuminators, may be gathered from the reſult of this experiment!

It appearing to me very probable, that the *difference in the whiteneſs of the two kinds of light*, which were the ſubjects of the foregoing experiments, might, ſome how or other, be the occaſion of the *different colours of the ſhadows*, I attempted to produce the ſame effects, by employing two artificial lights, of different colours; and in this I ſucceeded completely.

In a room, previouſly darkened, the light from two burning wax candles being made to fall upon the white paper at a proper angle, in order to form two diſtinct ſhadows of the cylinder, theſe ſhadows were found not to be in the leaſt coloured; but upon interpoſing a pane of yellow glaſs, approaching to a faint orange colour, before one of the candles, one of the ſhadows immediately became *yellow*, and the other *blue*.

When two ARGAND's lamps were made uſe of inſtead of the candles, the reſult was the ſame; the ſhadows were conſtantly, and very deeply coloured, the one yellow approaching to orange, and the other blue approaching to green. I imagined that the greeniſh caſt of this blue colour was owing, either to the want of whiteneſs of the one light, or to the orange hue of the other, which it acquired from the glaſs.

When equal panes of the ſame yellow glaſs were interpoſed before *both* the lights, the white paper took an orange hue, but the ſhadows were to all appearance *without the leaſt tinge of colour*; but *two* panes of the yellow glaſs being afterwards interpoſed before *one of the lights*, while only *one* pane remained

before

before the other, the colours of the ſhadows immediately returned.

The reſults of theſe experiments having confirmed my ſuſpicions, that the colours of the ſhadows aroſe from the *different degrees of whiteneſs* of the two lights, I now endeavoured, by bringing daylight to be of the ſame yellow tinge with candle light, by the interpoſition of ſheets of coloured glaſs, to prevent the ſhadows being coloured when daylight and candle light were, together, the ſubjects of the experiment; and in this I ſucceeded. I was even able to *reverſe* the colours of the ſhadows, by cauſing the daylight to be of a *deeper yellow* than the candle light.

In the courſe of theſe experiments, I obſerved, that different ſhades of yellow, given to the daylight, produced very different, and often quite unexpected effects; thus, one ſheet of the yellow glaſs, interpoſed before the beam of daylight, changed the yellow ſhadow to a lively violet colour, and the blue ſhadow to a light green; two ſheets of the ſame glaſs nearly deſtroyed the colours of both the ſhadows; and three ſheets changed the ſhadow which was originally yellow, to blue;—and that which was blue, to a purpliſh yellow colour.

When the beam of daylight was made to paſs through a ſheet of blue glaſs, the colours of the ſhadows,—the yellow as well as the blue,—were improved, and rendered in the higheſt degree clear, and brilliant; but when the blue glaſs was placed before the candle, the colours of the ſhadows were very much impaired.

In order to ſee what would be the conſequence of rendering the candle light of a ſtill deeper yellow, I interpoſed before it a ſheet of yellow, or rather orange-coloured glaſs, when a very unexpected and moſt beautiful appearance took place; the colour of the yellow ſhadow was changed to orange,—the blue ſhadow remained unchanged,—and the whole ſurface of the paper not covered by the ſhadows, appeared to be tinged of a moſt beautiful violet colour, approaching to a light crimſon or pink; almoſt exactly the ſame hue as I have often obſerved the diſtant ſnowy mountains and valleys of the Alps to take about ſunſet.

Is it not more than probable that this hue is, in both caſes, produced by nearly the ſame combinations of coloured light? In the one caſe, it is the white ſnow illuminated at the ſame time by the pureſt light of the heavens, and by the deep yellow rays from the weſt; and in the other, it is the white paper illuminated by broad daylight, and by the rays from a burning candle, rendered ſtill more yellow by being tranſmitted through the yellow glaſs.

The beautiful violet colour which ſpreads itſelf over the ſurface of the paper will appear to the greateſt advantage, if the pane of orange-coloured glaſs be held in ſuch a manner, before the candle, that only a part of the paper, half of it for inſtance, be affected by it, the other half of it remaining white.

To make theſe experiments with more convenience, the paper, which may be about 8 or 10 inches ſquare, ſhould be paſted, or glued down upon a flat piece of board, furniſhed with a ball and ſocket upon the hinder ſide of it, and mounted upon a ſtand;

a ſtand; and the cylinder ſhould be faſtened to a ſmall arm of wood, or of metal, projecting forward from the bottom of the board for that purpoſe. A ſmall ſtand, capable of being made higher or lower, as the occaſion requires, ſhould likewiſe be provided for ſupporting the candle; and if the board with the paper faſtened upon it be ſurrounded with a broad black frame, the experiments will be ſo much the more ſtriking and beautiful. For ſtill greater convenience, I have added two other ſtands, for holding the coloured glaſs through which the light is occaſionally made to paſs, in its way to the white ſurface upon which the ſhadows are projected. It will be hardly neceſſary to add, that in order to the experiments appearing to the greateſt advantage, all light, which is not abſolutely neceſſary to the experiment, muſt be carefully excluded.

Having fitted up a little apparatus according to the above directions, merely for the purpoſe of proſecuting theſe inquiries reſpecting the coloured ſhadows, I proceeded to make a great variety of experiments,—ſome with pointed views,—and others quite at random, and merely in hopes of making ſome accidental diſcovery that might lead to a knowledge of the cauſes of appearances, which ſtill ſeemed to me to be enveloped in much obſcurity and uncertainty.

Having found that the ſhadows correſponding to two like wax candles were coloured, the one blue, and the other yellow, by interpoſing a ſheet of yellow glaſs before one of them; I now tried what

the effect would be when *blue* glaſs was made uſe of inſtead of yellow, and I found it to be the ſame; the ſhadows were ſtill coloured, the one blue, and the other yellow, with this difference, however, that the colours of the ſhadows were *reverſed;* that which, with the yellow glaſs, was before yellow, being now blue, and that which was blue being yellow.

I afterwards tried a glaſs of a bright amethyſt colour, and was ſurpriſed to find that the ſhadows ſtill continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple caſt; but the blue, though a little inclining to green, was nevertheleſs a clean, bright, decided colour.

Having no other coloured glaſs at hand to puſh theſe particular inquiries farther, I now removed the candles, and opening two holes in the upper parts of the window-ſhutters of two neighbouring windows, I let into the room, from above, two beams of light, from different parts of the heavens, and, placing the inſtrument in ſuch a manner that two diſtinct ſhadows were projected by the cylinder upon the paper, I was entertained by a ſucceſſion of very amuſing appearances.

The ſhadows were tinged with an infinite variety of the moſt unexpected, and often moſt beautiful colours, which continually varying, ſometimes ſlowly, and ſometimes with inconceivable rapidity, abſolutely faſcinated the eyes, and commanding the the moſt eager attention, afforded an enjoyment as new as it was bewitching.

It was a windy day, with flying clouds, and it ſeemed, as if every cloud that paſſed, brought with

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it another complete ſucceſſion of varying hues, and moſt harmonious tints. If any colour could be ſaid to predominate it was purples; but all the varieties of browns, and almoſt all the other colours I ever remembered to have ſeen, appeared in their turns, and there were even colours which ſeemed to me to be perfectly new.

Reflecting upon the great variety of colours obſerved in theſe laſt experiments, many of which did not appear to have the leaſt relation to the apparent colours of the light by which they were produced, I began to ſuſpect that the colours of the ſhadows might, in many caſes, notwithſtanding their apparent brilliancy, be merely an optical deception, owing to contraſt, or to ſome effect of the other, *real*, and neighbouring colours upon the eye.

To determine this fact by a direct experiment, I proceeded in the following manner. Having, by making uſe of a flat ruler inſtead of the cylinder, contrived to render the ſhadows much broader, I ſhut out of the room every ray of daylight, and prepared to make the experiment with two ARGAND's lamps, well trimmed, and which were both made to burn with the greateſt poſſible brilliancy; and having aſſured myſelf that the light they emitted was preciſely of the ſame colour, by the ſhadows being perfectly colourleſs which were projected upon the white paper, I directed a tube of about 12 inches long, and near an inch in diameter, lined with black paper, againſt the centre of one of the broad ſhadows; and looking through this tube

with

with one eye, while the other was cloſed, I kept my attention fixed upon the ſhadow, while an aſſiſtant repeatedly interpoſed a ſheet of yellow glaſs before the lamp whoſe light correſponded to the ſhadow I obſerved,—and as often removed it.

The reſult of the experiment was very ſtriking, and fully confirmed my ſuſpicions with reſpect to the fallacy of many of the appearances in the foregoing experiments.

So far from being able to obſerve any change in the ſhadow upon which my eye was fixed, I was not able even to tell when the yellow glaſs was before the lamp, and when it was not; and though the aſſiſtant often exclaimed at the ſtriking brilliancy, and beauty of the blue colour, of the very ſhadow I was obſerving, I could not diſcover in it the leaſt appearance of any colour at all. But as ſoon as I removed my eye from the tube, and contemplated the ſhadow, with all its neighbouring accompaniments,—the other ſhadow rendered *really* yellow, by the effect of the yellow glaſs, and the white paper, which had likewiſe from the ſame cauſe, acquired a yellowiſh hue,—the ſhadow in queſtion appeared to me, as it did to my aſſiſtant, of a beautiful blue colour.

I afterwards repeated the ſame experiment with the apparently blue ſhadow produced in the experiment with daylight, and candlelight, and with exactly the ſame reſult.

How far theſe experiments may enable us to account for the apparent blue colour of the ſky, and the great variety of colours which frequently adorn

adorn the clouds; as alſo what other uſeful obſervations may be drawn from them, I leave to philoſophers, opticians, and painters to determine. In the mean time, I believe it is a new diſcovery, —at leaſt it is undoubtedly a very extraordinary fact,—that our eyes are not always to be believed, *even with reſpect to the preſence or abſence of colours.*

I cannot finiſh this paper without mentioning one circumſtance, which ſtruck me very forcibly, in all theſe experiments upon coloured ſhadows,—and that is, the moſt perfect harmony which always appeared to ſubſiſt between the colours,—whatever they were,—of the two ſhadows; and this harmony ſeemed to me to be full as perfect and pleaſing when the ſhadows were of different tints of brown, as when one of them was blue and the other yellow. In ſhort, the harmony of theſe colours was in all caſes not only very ſtriking, but the appearances altogether were quite enchanting; and I never found any body to whom I ſhewed theſe experiments whoſe eyes were not faſcinated with them. It is, however, more than probable, that a great part of the pleaſure which theſe experiments afforded to the ſpectators, aroſe from the continual changes of colour,—tint,—and ſhade, with which the eye was amuſed, and the attention kept awake.

We are uſed to ſeeing colours fixed, and unalterable,—hard as the ſolid bodies from which they come,—and juſt as motionleſs,—conſequently *dead, unintereſting, and tireſome to the eye;* but in theſe experiments all is *motion*,—*life*,—and *beauty*.

It appears to me very probable, that a further proſecution of theſe experiments upon coloured ſhadows, may not only lead to a knowledge of the *real nature of the harmony of colours*, or the peculiar circumſtances upon which that harmony depends; but that it may alſo enable us to conſtruct inſtruments for producing that harmony, for the entertainment of the eyes, in a manner ſimilar to that in which the ears are entertained by muſical ſounds. I know that attempts have already been made for that purpoſe; but when I conſider the means employed, I am not ſurpriſed that they did not ſucceed. Where the flowing tide,—the varying ſwell,—the *creſcendo* is wanting, colours muſt ever remain hard, cold, and inanimate maſſes.

I am very ſorry that my more ſerious occupations do not at preſent permit me to purſue theſe moſt entertaining inquiries. Perhaps, at ſome future period, I may find leiſure to reſume them.

CONJECTURES

RESPECTING THE

PRINCIPLES OF THE HARMONY OF COLOURS.

SINCE the foregoing paper was written, I have, at different times, repeated moſt of the experiments therein deſcribed; and have made a variety of others, with a view to the farther inveſtigation of this curious ſubject; and from the reſults of theſe inquiries, I have been enabled to form ſome concluſions and conjectures which may perhaps be thought not altogether unintereſting.

Whenever a beam of *coloured light* of any ſpecies, and a beam of *white* or *colourleſs light* of equal intenſity, arriving in different directions, and at equal angles of incidence at a plane white ſurface, illuminate that ſurface together, if a ſolid opaque body of any kind be placed, in each of theſe beams of light, juſt before the illuminated plane, in ſuch a manner that the two ſhadows caſt on the plane by theſe opaque bodies, may be near each other, the intenſities of theſe ſhadows will be equal, and they will both appear to be coloured, but of very different hues. That which is illuminated by the *coloured light* will be of the colour of that light;—which is what would naturally be expected to happen by a perſon who had never ſeen the experiment,—but that which is illuminated by the *colourleſs light*, and

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by that alone, instead of appearing colourless, will *appear* to be as deeply coloured as the other, but of a different hue.

The two colours exhibited by the two shadows appear in all cases to harmonize in the most perfect manner; or, in other words, to afford the most pleasing contrast to the view.

These two colours are always such, that if they could be intimately mixed together, the result of that mixture would be *perfect whiteness*;—and as whiteness results from the mixture of all the different colours, in certain proportions, the two shadows may be considered as containing all the colours in their just proportions; and the colour of the one shadow may with propriety be said to be the *complement* of the other.

Two neighbouring colours are then, and only then, in perfect harmony, when the intimate mixture of both would produce perfect whiteness; and hence it appears, that when two colours harmonize, one of them, at least, must necessarily be a compound colour.

In the experiment of the coloured shadows, the colour exhibited by one of the shadows only is real, that of the other is *imaginary*, being an optical deception, occasioned in some way unknown to us, by the colour actually present, and by the effects of the different lights and shades. The *imaginary colour*, which may be said to be *called up in the mind*, by the other *real colour*, does not, however, appear to be at all inferior to the real colour, either in lustre, or in the distinctness of its hue.

Any

Any two harmonizing coloured ſhadows may be produced indifferently, either with one of the given colours, or with the other of them, and white light: *pink* and *green*, for inſtance, are harmonizing colours, and two ſhadows of theſe two colours, equally bright, may be produced, either with a beam of pink-coloured light, or with a beam of green light, croſſed by a beam of white light; according to the method above deſcribed.

A beam of coloured light may readily be produced for making theſe experiments by cauſing white light to paſs through coloured glaſs, or any other coloured tranſparent ſubſtance.

To every colour without exception, whatever may be its hue, or ſhade, or however it may be compounded, there is another in perfect harmony to it, which is its complement, and may be ſaid to be its companion. It may be *called up* and exhibited to view in the following manner. Let white light be made to paſs through the coloured body; or, if it be opaque, let it be reflected from it; with this light, ſo coloured, and with pure white light, make the experiment of the two ſhadows; and the colour in queſtion will appear *with its companion by its ſide.*

By experiments of this kind, which might eaſily be made, ladies may chuſe ribbons to their gowns; or thoſe who furniſh rooms may arrange their colours, upon principles of the moſt perfect harmony, and of the pureſt taſte.

The advantages that painters might derive from a knowledge of theſe principles of the harmony of colours, are too obvious to require illuſtration.

Upon

Upon a careful examination of the works of the great maſters of the art of colouring, it will appear that they have frequently practiſed upon theſe principles, though it is not likely that they were acquainted with the ſcientific foundation of their practice. They have certainly produced *appearances* of colours, or tints, when their pictures are viewed in a proper light, and at a proper diſtance, which we ſearch for in vain, upon the canvas. This may well be called the "*Magic of Colouring*;" for it is in fact calling up, as by enchantment, and preſenting to the mind, colours the moſt pure, and vivid, which have no real exiſtence.

As it might very naturally be ſuſpected that the colours called up by means of ſhadows, owe their exiſtence to *ſomething peculiar to ſhadows*, and that ſimilar effects could not be produced without ſhadows, by means of coloured pigments; to remove all doubts on that ſubject, I made the following deciſive experiment.

Having found that when a beam of deep red light, and a beam of white or colourleſs light, of equal intenſity, arrive in different directions, at a plane white ſurface, and illuminate it; that a blue ſhadow, nearly approaching to green, is called up by the red ſhadow, I attempted to imitate this experiment with a coloured pigment.

On the middle of the floor of a ſpacious room, I laid down a very large ſheet of black paper, and on the middle of this, I placed a circular piece of crayon paper, which, in order that it might ſupply the place of the illuminated plane ſurface on which the

ſhadows were projected in my experiments, I covered, or coloured it with ſuch a mixture of red-lead *(minium)*, and pure white lead, both finely powdered and well mixed together, as brought it to be of the ſame tint, as nearly as poſſible, with the ſurface illuminated by the red, and by the white light. I then took two oblong ſlips of crayon paper, half an inch wide, and two inches long each; then, colouring one of them, as highly as poſſible, with red-lead, in a dry powder, and covering the other with a powder compoſed of white lead and lamp-black, in ſuch proportions that the quantities of light reflected from the two ſlips, ſo prepared, ſhould be equal, I placed theſe ſlips, in contact with each other, in the middle of the circular piece of paper on the floor; when, retiring backwards a few ſteps, and looking through my hand, with one eye, to exclude all other objects, I had the pleaſure to perceive that the ſlip of paper which was covered with a grey powder, now appeared to be of a beautiful greeniſh blue colour, while the other was of the moſt vivid red.

This experiment was firſt made at an inn at Florence, in the year 1793; and in order that I might aſſure myſelf that my expectations had not deceived me by impoſing upon my ſenſes, I called two of my friends who happened to lodge in the houſe (Lord and Lady Palmerſton) into the room, and without letting them into the ſecret, ſimply aſked them, with a feigned air of indifference, which of the two colours they ſaw in the centre of the circular piece of paper on the floor they thought the brighteſt.

After looking at them for ſome time, and going round to view them from different ſides, one of them anſwered,—" I don't know which of them is the brighteſt.—The red is very bright, and ſo is the blue.—But why do you aſk us that queſtion?"

When I told them that there was no blue there, and that what they took to be blue was merely a deception, they did not believe me; but they were much ſurpriſed, and convinced that what I told them was true, when they ſaw, on my removing the red ſlip, that its companion, which was left behind, inſtantly *faded*, and *loſt its colour*.

In attempts to call up colours in this way, many precautions are neceſſary, to which the moſt ſcrupulous attention muſt be paid, otherwiſe the experiments will not ſucceed. Care muſt be taken to exclude all coloured light in illuminating the ſlips of paper, and in preparing that ſlip which is deſigned for exhibiting the *imaginary* colour, the quantities of black and of white powder that are mixed, muſt be ſo adjuſted to each other, that when the ſurface of the ſlip is covered with it, the *quantity* of light reflected from it to the ſpectator's eye, muſt be preciſely equal to that reflected from the ſurface of the other, *coloured ſlip*, for this equality is eſſential to the purity and brilliancy of the colour called up. But this equality can only be found by actual trials, with ſeveral ſlips of deeper and lighter ſhades. That ſlip which takes the cleareſt and brighteſt colour is to be choſen.

When

When experiments of this kind are attempted to be made with oil-colours, other, and ſtill greater difficulties will occur; for the oil uſed for fixing the colours diminiſhes in ſo great a degree, both the brilliancy, and the purity, of the light reflected from the ſurfaces of coloured pigments, that the light reflected from an oil painting cannot be expected to produce the ſame brilliant appearances which are exhibited by the mixtures and contraſts of the uncontaminated and brilliant colours of pure light.

But although it may be impoſſible for painters, with *their imperfect colours*, to produce effects that will bear a cloſe compariſon with thoſe magic appearances, of which we have been giving an account; yet there can be no doubt but that the knowledge of thoſe facts, and of the theory by which they are explained, may be very uſeful to them.

The impoſſibility of producing perfect whiteneſs by any mixture of painters' colours, is a proof of the want of purity of thoſe colours; and of the difficulty of imitating by means of them any of thoſe very ſtriking effects, which are exhibited in experiments with the pure priſmatic colours.

There is one moſt important advantage which painters may certainly derive from a knowledge of the principles of the harmony of colours: it will enable them, on ſound philoſophical principles, to contraſt their colours in ſuch a manner, as to give to their pictures, or rather, to what they chuſe to make the prominent parts of them, a great degree of force and brilliancy. For, if any, and every ſimple and compound colour has ſuch a

power on objects near it as to cauſe a neighbouring *colourleſs ſhadow* to aſſume the appearance of a colour, there can be no doubt but that, if, inſtead of the ſhadow, a *real colour*, nearly of the ſame tint and ſhade, as that ſo *called up*, be ſubſtituted in its place, *this colour will appear to great advantage*, or will aſſume an uncommon degree of ſtrength and brightneſs.

The ſcience of painting is a moſt curious and intereſting ſubject of philoſophical inveſtigation; and until it is more cultivated the art of colouring muſt continue to be very obſcure, uncertain, and imperfect. Genius will be condemned to waſte its energy in tedious mechanical experiments, inſtead of being employed, as it ought to be, in tracing with a rapid pencil, the beautiful conception of a ſublime imagination.

AN INQUIRY

CONCERNING

THE CHEMICAL PROPERTIES

THAT HAVE BEEN ATTRIBUTED TO

LIGHT.

[Read before the Royal Society, June 14, 1798.]

IN the Second Part of my Seventh Eſſay, (on the propagation of heat in fluids,) I have mentioned the reaſons which had induced me to doubt of the exiſtence of thoſe chemical properties in light, that have been attributed to it, and to conclude, that all thoſe viſibles changes which are produced in bodies by expoſure to the action of the ſun's rays, are effected, not by any chemical combination of the matter of light with ſuch bodies, but merely by the heat which is generated, or excited, by the light that is abſorbed by them.

As the deciſion of this queſtion is a matter of great importance to the advancement of ſcience, and particularly to chemiſtry, and as the ſubject is in many reſpects curious and intereſting, it has often employed my thoughts in my leiſure hours; and I have ſpent much time in endeavouring to contrive experiments, from the unequivocal reſults of which the truth might be made to appear. Though I have not been ſo ſucceſsful in theſe inveſtigations as I could wiſh, yet, I cannot help flattering myſelf,

that an account of the refults of fome of my late experiments will be thought fufficiently curious and intereſting to merit the attention of thoſe who take pleaſure in the cultivation of experimental philoſophy.

Having found that gold, or ſilver, might be melted by the heat (inviſible to the ſight) which exiſts in the air, at the diſtance of more than an inch above the point of the flame of a wax candle, (ſee my Seventh Eſſay, Part II. page 350.) I was curious to know what effect this heat would produce on the oxides of thoſe metals.

Experiment, N° 1. Having evaporated to dryneſs a ſolution of fine gold in nitro-muriatic acid, I diſſolved the reſiduum, in juſt as much diſtilled water as was neceſſary in order that the ſolution, (which was of a beautiful yellow colour) might not be diſpoſed to cryſtallize; and, wetting the middle of a piece of white taffeta riband, $1\frac{1}{2}$ inch wide, and about 8 inches long, in this ſolution, I held the riband, (with both my hands) ſtretched horizontally over the clear bright flame of a wax candle; the under ſide of the riband being kept at the diſtance of about $1\frac{1}{2}$ inch above the point of the flame. The reſult of this experiment was very ſtriking. That part of the riband which was directly over the point of the flame, began almoſt immediately to emit ſteam in denſe clouds; and, in about 10 ſeconds, a circular ſpot, about $\frac{3}{4}$ of an inch in diameter, having become nearly dry, a ſpot of a very fine purple colour, approaching to crimſon, ſuddenly made its appearance in the middle of it, and ſpreading

ing rapidly on all ſides, became, in one or two ſeconds more, nearly an inch in diameter.

By moving the riband, ſo as to bring, in their turns, all the parts of it which had been wetted with the ſolution to be expoſed to the action of the current of hot vapour that aroſe from the burning candle, all thoſe parts which had been ſo wetted, were tinged with the ſame beautiful purple colour.

This colour, which was uncommonly brilliant, paſſed quite through the riband, and I found the ſtain to be perfectly indelible. I endeavoured to waſh it out; but nothing I applied to it, appeared in the ſmalleſt degree to diminiſh its luſtre. The hue was not uniform, but varied from a light crimſon to a very deep purple, approaching to a reddiſh brown.

I ſearched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the moſt beautiful purple enamel, which, in the ſun, had a degree of brilliancy that was ſometimes quite dazzling.

Experiment, N° 2. A piece of the riband which had been wetted with the aqueous ſolution of the oxide, was carefully dried in a dark cloſet, and was then expoſed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the ſolution, (and which on drying had acquired a faint yellow colour,) was tinged of the ſame bright purple colour as was produced in the

laſt-mentioned experiment, when the riband was expoſed wet to the action of the heat.*

Experiment, N° 3. A piece of the riband which had been wetted with the ſolution, and dried in the dark, was now wetted with diſtilled water, and expoſed *wet* to the action of the aſcending current of hot vapour which aroſe from the burning candle: the purple ſtain was produced as before, which extended as far as the riband had been wetted with the ſolution, but no farther.

I afterwards varied this experiment in ſeveral ways, ſometimes uſing paper, ſometimes fine linen, and ſometimes fine cotton cloths, inſtead of the ſilk riband; but nearly the ſame tinge was produced, whatever the ſubſtance was that was made to imbibe the aqueous ſolution of the metallic oxide.

Similar experiments, and with ſimilar reſults, were likewiſe made with pieces of riband, fine linen, cotton, paper, &c. wetted in an aqueous ſolution of nitrate of ſilver; with this difference, however, that the tinge produced by this metallic oxide, inſtead of being of a deep purple, inclining to a crimſon, was of a very dark orange colour, or rather of a yelloweiſh brown.

In order to diſcover whether the purple tinge, in the experiments with the oxide of gold, was occaſioned by the *heat* communicated by the aſcending

* We ſhall hereafter find reaſon to conclude, that the ſucceſs of this experiment, or the appearance of the purple tinge, was owing to the watery vapour or ſteam which exiſted in the hot current of vapour that aſcended from the flame of the candle.

current

current of hot vapour, or by the *light* of the candle, I made the following experiment, the result of which I conceive to have been decisive.

Experiment, N° 4. A piece of riband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame.

The riband was dried, but its colour was not in the smallest degree changed.

When it was held a few seconds within about $\frac{1}{8}$ of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced.

The heat which existed at that distance from the flame, *on the side of it*, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flatted, such as is used in making silver lace.

The objects I had in view in the following experiments, will be too evident to require any particular explanation.

Experiment, N° 5. Two like pieces of riband were wetted at the same time in the solution, and suspended, while wet, in two thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed, naked, to the direct rays of a bright sun; while B was inclosed in a cylinder of pasteboard, painted black within and without, and closed with a fit

cover,

cover, and conſequently remained in perfect darkneſs.

In a very few minutes, the riband in the phial A began ſenſibly to change its colour, and to take a purple hue; and, at the end of five hours, it had acquired a deep crimſon tint throughout.

The phial B was expoſed in the window, in its dark cylindrical cover, three days; but there was not the ſmalleſt appearance of any change of colour in the ſilk.

Experiment, N° 6. Two ſmall parcels of *magneſia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a ſhilling,) were placed in heaps, in two china plates, A and B, and thoroughly moiſtened with the before-mentioned aqueous ſolution of the oxide of gold. Both plates were placed in the ſame window; the moiſtened earth in the plate A being expoſed naked to the ſun's rays; while that in the plate B was exactly covered with a tea-cup, turned upſide down, which excluded all light.

The *magneſia alba* in the plate A, which was expoſed to the ſtrong light of the ſun, began almoſt immediately to change colour, taking a faint violet hue, which by degrees became more and more intenſe, and in a few hours ended in a deep purple; while that in the plate B, which was kept in the dark, retained the yellowiſh caſt it had acquired from the ſolution, without the ſmalleſt appearance of change.

Experiment, N° 7. A ſmall parcel of *magneſia alba*, placed on a china plate, having been moiſtened

ened with the aqueous ſolution of the oxide of gold, and thoroughly dried in a dark cloſet, was now expoſed, *in this dry ſtate*, to the action of the direct rays of a very bright ſun.

It had been expoſed to this ſtrong light above half an hour, before its colour began to be *ſenſibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with diſtilled water, it changed colour very rapidly, and ſoon came to be of a deep purple tint, approaching to crimſon.

Experiment, N°. 8. A piece of white taffeta riband, which had been wetted with the ſolution, and thoroughly dried in the dark, was ſuſpended in a clean dry phial of very fine tranſparent glaſs; and the phial, being well ſtopped with a dry cork, was expoſed to the ſtrong light of a bright ſun.

After the riband had been expoſed, in this manner, to the action of the ſun's direct rays about half an hour, there were here and there ſome faint appearances of a change of its colour; but it ſhewed no diſpoſition to take that deep purple hue which the riband had always acquired, when expoſed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with diſtilled water, and expoſing it again, *while thus wetted*, to the ſun's rays, it almoſt inſtantaneouſly began to change colour, and ſoon became of a deep purple tint; but, though I examined the ſurface of the riband with the utmoſt care, and with a good lens, both during the expe-

riment, and after it, I could not perceive the ſmalleſt particle of *revived gold*, nor did I ſee any veſtige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated ſeveral times, and always with reſults which led me to conclude, (what indeed was reaſonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a ſtate of cryſtallization.*

The heat which is generated by the abſorption of the rays of light muſt neceſſarily, *at the moment of its generation* at leaſt, exiſt in almoſt infinitely ſmall ſpaces; and conſequently, it is only in bodies that are *inconceivably ſmall* that it can produce durable effects, in any degree indicative of its extreme intenſity.

Perhaps the particles of the oxide of gold, diſſolved in water, are of ſuch dimenſions; and it is very remarkable, that the colours produced, in ſome of my experiments on white ribands, by means of an aqueous ſolution of the oxide of gold, are precifely the ſame as are produced from the oxide of that metal, by enamellers, in the intenſe heat of their furnaces.

As the colouring ſubſtance is the ſame, and as the colours produced are the ſame, why ſhould we not conclude that the effects are produced in both theſe caſes by the ſame means, that is to ſay, by the agency of heat? or, in other words, and to be more explicit, by expoſing the oxide in a certain tem-

temperature, at which it becomes diſpoſed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the reſults of the following experiments afford ſtill more ſatisfactory information, reſpecting the intenſity of the heat generated in all caſes where light is abſorbed; and the ſtriking effects, which, under certain circumſtances, it is capable of producing.

The facility with which moſt of the metallic oxides are reduced, in the dry way, by means of charcoal, ſhews, that, at a certain (high) temperature, oxygen is diſpoſed to quit thoſe metals, in order to form a chemical union with the charcoal; or at leaſt with ſome one of its conſtituent principles, if it be a compound ſubſtance; and hence I concluded, that gold might be revived, *in the moiſt way*, by means of charcoal, from a ſolution of its oxide in water, were it poſſible, under ſuch circumſtances, to communicate to the charcoal, and to the oxide, *at the ſame time*, a degree of heat ſufficient for that purpoſe.

To ſee if this might not be done by means of light, I made, or rather repeated, the following very intereſting experiment.

Experiment, N° 9. Into a thin tube of very fine colourleſs glaſs, 10 inches long, and $\frac{6}{10}$ of an inch in diameter, cloſed hermetically at its lower end, I put as many pieces of charcoal, about the ſize of large peas, as filled the tube to the height of two inches; and, having poured on them as much of

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the aqueous ſolution of nitro-muriate of gold as nearly covered them, expoſed the tube, with its contents, to the action of the direct rays of a very bright ſun.

In leſs than half an hour, ſmall ſpecks of revived gold, in all its *metallic ſplendour*, began to make their appearance, here and there, on the ſurface of the charcoal; and, in ſix hours, the ſolution, which at firſt was of a bright yellow colour, became perfectly *colourleſs*, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The ſurface of the charcoal was, in ſeveral places, nearly covered with ſmall particles of revived gold; and the inſide of the glaſs tube, in that part where it was in contact with the upper ſurface of the contained liquid, was moſt beautifully gilded.

This gilding of the tube was very ſplendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greeniſh blue colour, without the ſmalleſt appearance of any metallic ſplendour.

From the colour, and apparent denſity of this cloud, I was induced to conclude, that the gilding on the glaſs was leſs than *one millionth part of an inch* in thickneſs.

This intereſting experiment was repeated ſix times, and always with nearly the ſame reſult. The gold was completely revived in each of them, and the ſolution left perfectly colourleſs; in moſt

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of the experiments, however, the ſides of the glaſs were not gilded, all the revived gold remaining attached to the ſurface of the charcoal.

In two of theſe experiments, I made uſe of pieces of charcoal which had been previouſly boiled ſeveral hours in a large quantity of diſtilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the ſolution, to prevent them from imbibing any air; and, in different experiments, the ſolution was uſed of different degrees of ſtrength.

I plainly perceived that the experiment ſucceeded beſt, that is to ſay, that the gold was *ſooneſt revived*, in thoſe caſes in which the ſolution was *moſt diluted:* one of the experiments, however, and which ſucceeded perfectly, was made with the ſolution ſo much condenſed, that it was nearly at the point at which it became diſpoſed to cryſtallize *.

On examining, with a good microſcope, the particles of revived gold which remained attached to the ſurface of the charcoal, after it had been dried, I found them to conſiſt of an infinite number of ſmall ſcales, ſeparated from each other; not very highly poliſhed, but poſſeſſing the true metallic ſplendour, and a very deep and rich gold colour.

* This agrees perfectly with the reſults of ſimilar experiments made by the ingenious and lively Mrs. FULHAME. (See her Eſſay on Combuſtion, page 124.)

It was on reading her book, that I was induced to engage in theſe inveſtigations; and it was by her experiments, that moſt of the foregoing experiments were ſuggeſted.

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The gold which attached itſelf to the inſide of the glaſs tube, was in the form of a ring, about $\frac{1}{10}$ of an inch wide, (badly defined however below,) and adhered to the glaſs with ſo much obſtinacy, as not to be removed by rincing out the tube a great number of times with water; it had, as has already been obſerved, a very high poliſh, when ſeen by reflected light.

Thoſe who enter into the ſpirit of theſe inveſtigations, will eaſily imagine how impatient I muſt have been, after ſeeing the reſults of theſe experiments, to find out whether gold could be revived from this aqueous ſolution of its oxide by means of charcoal, *without the aſſiſtance of light*, and merely by ſuch a degree of equal heat as could be given to it in the dark. To determine that important queſtion, the following experiment was made.

Experiment, N° 10. A cylindrical glaſs tube, $\frac{6}{10}$ of an inch in diameter, and 10 inches long, cloſed hermetically at its lower end, and containing a quantity of a diluted aqueous ſolution of the oxide of gold, mixed with charcoal in broken pieces, about the ſize of large peas, was put into a fit cylindrical tin caſe, which was nicely cloſed with a fit cover; and the glaſs tube, with its contents, ſo ſhut up in the dark, was expoſed two hours, in the temperature of 210° of FAHRENHEIT'S ſcale.

On taking the glaſs tube out of its tin caſe, I found the ſolution *perfectly colourleſs*, and the revived gold adhering to the ſurface of the charcoal.

On repeating the experiment, and uſing the ſolution nearly ſaturated with the oxide, the reſult was preciſely the ſame; the ſolution being found perfectly colourleſs, and the revived gold adhering to the ſurface of the charcoal.

I own fairly, that the reſults of theſe laſt experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypotheſis, reſpecting the cauſes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypotheſis of mine, I hope and truſt that I never ſhall be ſo weak as to feel pain at the diſcovery of truth, however contrary it may be to my expectations; and ſtill leſs, to feel a ſecret wiſh to ſuppreſs experiments, merely becauſe their reſults militate againſt my ſpeculative opinions.

It is proper I ſhould obſerve, that the charcoal uſed in this laſt-mentioned experiment had been boiled two hours in diſtilled water, by which means its pores had been ſo completely filled with that fluid, that the pieces of it that were uſed were ſpecifically heavier than water, and ſunk in it to the bottom of the containing veſſel.

Having been ſo ſucceſsful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moiſt way*, I loſt no time in making ſimilar experiments with the oxide of ſilver.

Experiment, N° 11. A ſolution of fine ſilver, in nitrous acid, was evaporated to dryneſs, and the reſiduum diſſolved in diſtilled water.

A portion of this ſolution, (which was perfectly colourleſs,) diluted with twice as much diſtilled water, was poured into a phial containing a number of ſmall pieces of charcoal; and the phial, being well cloſed with a new cork ſtopple, was expoſed to the action of the ſun's rays.

In leſs than an hour, ſmall ſpecks of revived ſilver began to make their appearance on the ſurface of the charcoal; and, at the end of two hours, theſe ſpecks became very numerous, and had increaſed ſo much in ſize, that they were diſtinctly viſible to the naked eye at the diſtance of more than three feet. They were very white, and poſſeſſed the metallic ſplendour of ſilver in ſo high a degree, that when enlightened by the ſun's beams, their luſtre was nearly equal to that of very ſmall diamonds.

The phial which was in the form of a pear, and about $1\frac{1}{2}$ inch in diameter at its bulb, was very thin, and made of very fine colourleſs glaſs; the aqueous ſolution was alſo perfectly tranſparent and colourleſs; and, when the contents of the phial were illuminated by the direct rays of a bright ſun, the contraſt of the white colour of theſe little metallic ſpangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and intereſting ſight.

As the air had been previouſly expelled from the charcoal, by boiling it in diſtilled water, it was ſpecifically heavier than the aqueous ſolution of the metallic oxide, and conſequently remained at the bottom of the bottle.

Expe-

Experiment, N° 12. A phial as nearly as poſſible like that uſed in the laſt experiment, and containing the ſame quantity of diluted aqueous ſolution of nitrate of ſilver, and alſo of charcoal, was incloſed in a cylindrical tin box, and expoſed one hour to the heat of boiling water, in an apparatus uſed for boiling vegetables in ſteam, for the table.

The reſult of this experiment was uncommonly ſtriking. The ſurface of the charcoal was covered with a moſt beautiful *metallic vegetation*; ſmall filaments of revived ſilver, reſembling fine flatted ſilver wire, puſhing out from its ſurface, in all directions!

Some of theſe metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were eaſily detached from the ſurface of the charcoal, to which they ſeemed to adhere but very ſlightly.

Theſe experiments were repeated ſeveral times, and always with preciſely the ſame reſults.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of ſmall ſcales, adhering firmly to the ſurface of the charcoal, as has already been obſerved.

The following experiments, which were firſt ſuggeſted by an accident, were made with a view to inveſtigate ſtill farther, the cauſes of thoſe effects which have been attributed to the ſuppoſed chemical properties of light.

Having accidentally put away two ſmall phials, each containing a quantity of aqueous ſolution of the oxide of gold and ſulphuric ether, in each of which the ether had extracted the gold completely

from the ſolution, as was evident by the yellow colour of the ſolution having been transferred to the ether, and the ſolution being left colourleſs; in one of the phials, which happened to ſtand in a window, in which there was occaſionally a ſtrong light, (though the direct rays of the ſun never fell upon it,) I found, in about three weeks, the oxide of gold was almoſt entirely reduced; the revived gold appearing in all its metallic ſplendour, in the form of a thin pellicle, ſwimming on the ſurface of the aqueous liquor in the phial, and the colour of the ether which repoſed on it having become quite faint; while no viſible change had been produced in the contents of the other phial, which had ſtood in a dark corner of the room.

As theſe appearances induced me to ſuſpect, or rather ſtrengthened the ſuſpicions I had before conceived, that the ſeparation of gold from ether under its metallic form, when a ſolution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther inveſtigation of that matter.

Experiment, N° 13. Into a ſmall pear-like phial, of very fine tranſparent glaſs, I put equal quantities of an aqueous ſolution, a cryſtallized oxide of gold, and of ſulphuric ether; and the phial, which was about half filled, being cloſed with a good cork, well ſecured in its place, was expoſed to the action of the direct rays of a bright ſun.

A pellicle of revived gold, in all its metallic

ſplendour,

ſplendour, began almoſt immediately to be formed on the ſurface of the aqueous liquid, and ſoon covered it entirely; and, at the end of two hours, the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourleſs*.

On ſhaking the phial the metallic pellicle which covered the ſurface of the aqueous liquid was broken into ſmall pieces, which had exactly the appearance of leaf gold, poſſeſſing the true colour, and all the metallic brilliancy of that metal.

On ſuffering the phial to ſtand quiet, the aqueous liquor, and the ether, ſeparated, and moſt of the broken pieces of the thin ſheet of gold deſcended to the bottom of the phial: the remainder of them floated on the ſurface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly tranſparent and *colourleſs*.

By the length of time which was required for the ether and the aqueous liquid to ſeparate, I thought I could perceive that the ether had loſt ſomething of its fluidity; but, as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I ſaw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been ſuffered to cool, there was no appearance of any conſiderable quantity of air, or other permanently elaſtic fluid, having been either generated or abſorbed, during the experiment.

Finding that the oxide of gold might be ſo completely and ſo expeditiouſly reduced, by means of

ether, I conceived it might be poſſible to perform that chemical proceſs, *in the moiſt way*, by means of eſſential oils; and this conjecture proved to be well founded.

Experiment, N° 14. Upon a quantity of a diluted aqueous ſolution of nitro-muriate of gold, in a ſmall pear-like phial, about $1\frac{1}{2}$ inch in diameter at its bulb, was poured a ſmall quantity of etherial oil of turpentine, juſt as much as was ſufficient to cover the aqueous ſolution to the height of $\frac{2}{10}$ of an inch; and the phial being cloſed with a good cork, well ſecured in its place, it was expoſed one hour to the heat of boiling water in a ſteam-veſſel.

The gold was revived, appearing in the form of a ſplendid pellicle of a bright gold colour, which floated on the ſurface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourleſs as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it repoſed, had entirely loſt its yellow colour.

On ſhaking the phial, its contents were intimately mixed; but, on ſuffering it to ſtand quiet, the oil of turpentine ſoon ſeparated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourleſs.

On ſhaking the phial, *before it had been expoſed to the heat*, and mixing its contents, and then ſuffering it to ſtand quiet, the oil of turpentine, on taking its place at the top of the aqueous ſolution, was not found to have acquired any colour; nor was the bright gold colour of the ſolution

found to be at all impaired. When ſulphuric ether was uſed, inſtead of the oil of turpentine, the effect was in this reſpect very different.

To find out whether the oil of turpentine uſed in this experiment, and which had acquired a deep yellow colour, had loſt that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous ſolution of the oxide into the phial, and ſhaking the phial, expoſed it, with its contents, to the heat of boiling water.

After it had been expoſed to this heat about two hours, I examined it, and found, that though a conſiderable quantity of gold had been revived, yet the aqueous liquid ſtill retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial, I now added about half as much diſtilled water, and, mixing the whole by ſhaking, I expoſed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourleſs*.

On repeating this experiment with oil of turpentine, and varying it, by uſing a ſolution of the oxide of *ſilver*, (an aqueous ſolution of nitrate of ſilver,) inſtead of that of *gold*, the reſult was nearly the ſame; the metal was revived, and the oil of turpentine acquired a faint greeniſh yellow colour.

I alſo revived the oxides of gold and of ſilver with *oil of olives*, by a ſimilar proceſs, with the

heat of boiling water. The oil of olives uſed in theſe experiments loſt its tranſparency, and became deeply coloured; that uſed in the reduction of the oxide of ſilver, taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold, being changed to a yellowiſh brown with a purple hue.

In the experiment with the oxide of ſilver, the inſide of the phial, in the region where the oil repoſed on the aqueous ſolution was beautifully ſilvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and in both experiments, ſmall detached pellicles of revived metal were viſible in the oil, and adhered in ſeveral places to the inſide of the phial, forming bright ſpots, in which the colour of the metal, and its peculiar ſplendour, were perfectly conſpicuous.

Experiment, N° 15. As *carbon* is one of the conſtituent principles of ſpirit of wine, as well as of eſſential oils and ſulphuric ether, I thought it poſſible that I might ſucceed in the reduction of the oxide of gold, by mixing alcohol, with an aqueous ſolution of nitro-muriate of gold, and expoſing the mixture, in a phial well cloſed, to the heat of boiling water; but the experiment did not ſucceed.

By pouring upon this mixture a ſmall quantity of oil of olives, and expoſing it again to the heat of boiling water, the gold was revived.

Is it not probable, that the reaſon why the oxide was not reduced by alcohol, is the mobility of thoſe elements, which ought to act on each other, in order that the effect in queſtion may be produced?

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There is reaſon to think the oxide would be reduced, could the alcohol be made to reſt on the ſurface of the aqueous ſolution, without mixing with it.

I wiſhed to have been able to collect and examine the elaſtic fluids, which probably were formed in moſt of the preceding experiments; but my time was ſo much taken up with other matters, that I had not leiſure to purſue theſe inveſtigations farther.

In order to ſee what effects would be produced by the heat generated at the ſurface of an opaque body, of a nature different from thoſe hitherto uſed in the reduction of the metallic oxides, and one that is little diſpoſed to form a chemical union with oxygen, *(magneſia alba,)* when, being immerſed in an aqueous ſolution of the oxide of gold, the rays of the ſun were made to impinge on it, I contrived the following experiment.

Experiment, N° 16. I took four ſmall thin phials, A, B, C, and D, of very fine glaſs, and, putting into each of them about five grains of dry *magneſia alba*, I filled the phial A, nearly full, with a ſaturated aqueous ſolution of the oxide of gold.

I filled the phial B, in like manner, with ſome of the ſame ſolution, diluted with an equal quantity of diſtilled water; and the phials C and D were filled with the ſolution ſtill farther diluted.

Theſe phials, open, or without ſtoppers, were expoſed one whole day to the action of the direct rays of a bright ſun, their contents being often well mixed together, during that time, by ſhaking.

The contents of all theſe phials changed colour, more or leſs, but they acquired very different hues.

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The contents of the phial A became of a very deep rich *gold colour*, approaching to *orange*, the earthy ſediment being throughout of the ſame tint.

The contents of the phial B, which were at firſt of a light ſtraw colour, firſt changed to a light green, and then to a greeniſh blue. The phial having been ſuffered to ſtand quiet ſeveral days, in an uninhabited room, in a retired part of the houſe, the ſolution became nearly colourleſs, and the ſediment was found to be of a dirty olive colour.

The colour of the contents of the phials C and D was changed nearly in the ſame manner; and, having been ſuffered to ſtand quiet two or three days, to ſettle, the ſolution was found to be quite colourleſs, and the ſediment to be deeply coloured. There was, however, a very remarkable difference in the hues of the contents of the two phials; that of the phial C being of a light greeniſh blue; while that in the phial D was indigo, and of ſo deep a tint, that it might eaſily have been taken for black.

Theſe appearances were certainly very ſtriking, and well calculated to excite curioſity; I wiſh that what I have done may induce others to purſue theſe intereſting inveſtigations.

SUPPLEMENT.

SUPPLEMENT.

Since the foregoing paper was prefented to the Royal Society, I have had an opportunity of profecuting thefe inquiries a little farther; and the refults of two of my late experiments were fo remarkable that I have thought them deferving of being made known to the public.

Experiment, N° 17. Into a thin globe of fine colourlefs glafs, about 1½ inches in diameter, with a fhort cylindrical neck, I put equal parts of a weak folution of gum arabic in water, and of a diluted aqueous folution of the oxide of gold, and filling the globe about two thirds full with thefe liquids, which, being well mixed together by fhaking, the globe was fufpended to a nail, by its neck, near a window, in an unfrequented room fronting the north; where, by accident, it happened to remain undifturbed and unobferved, fix weeks.

When the globe was examined, it exhibited a very curious appearance. The glafs was beautifully tinged in every part where it had been in contact with the liquid, but the hues were very different in different parts. The part of the globe in contact with the upper furface of the liquid, was of a very faint purple, but this tinge gradually became of a deeper colour as it defcended by the fides of the globe, and ended below in a rich gilding, which had all the metallic fplendour of pure gold.

Experiment, N° 18. Having provided a thin flip of ivory, about half an inch wide, and 3 inches long,

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I introduced it into a ſmall phial, with a wide mouth, nearly filled with a diluted ſolution of nitrate of ſilver, where it was ſuffered to remain, in a dark cloſet, till the ivory had acquired a bright yellow colour. The ſlip of ivory was then taken out of the phial, and immerſed in a tumbler of pure water, and immediately expoſed, in the water, to the direct rays of a bright ſun.

The inſtant the ſun beams fell upon the ivory it began to change colour, and in leſs than two minutes, from being of a very beautiful yellow, it became quite black.

The rapidity with which this change of colour takes place is very ſtriking, and renders the experiment uncommonly intereſting. On examining the ivory its ſurface was found to be covered with a fine coaly ſubſtance, which was eaſily rubbed off with the hand. On removing this coaly ſubſtance, after the ivory had been ſuffered to remain two or three hours expoſed in water to the action of the ſun's light, the ſurface of the ivory was found to be completely ſilvered over, ſo as perfectly to reſemble a ſlip of metal.

Although this coating of revived metal, which covers the ſurface of the ivory, is very thin, yet, if the ivory be well ſoaked in the ſolution of nitrate of ſilver, the oxide of that metal will penetrate the ivory to a conſiderable depth, and, as faſt as the ſilvering wears off from the ſurface of the ivory, the oxide below it being uncovered, and expoſed to the light, a new coating of revived metal will be formed to replace it, and the ſurface of the ivory will not loſe its metallic appearance.

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I tried, by a ſimilar proceſs, to gild a ſlip of ivory with gold, but in this attempt I did not ſucceed as well as I could have wiſhed. A ſlip of ivory, which had been ſoaked in a diluted ſolution of oxi-muriate of gold, did not at firſt acquire a metallic appearance on being expoſed in water to the action of the ſun's rays; but I found, on examining one of theſe ſlips after it had been laid by for ſeveral months, that its ſurface was ſlightly gilded.

I think it highly probable, that means may be deviſed for expediting this proceſs, and gilding ivory, and perhaps ſome other ſubſtances, in this way; which would be a valuable acquiſition to the arts.

This method of ſilvering ivory, which is not only expeditious, but very economical, might, no doubt, be employed with advantage in many caſes, for ornamental purpoſes. The proceſs is certainly curious, when conſidered merely as a philoſophical experiment; and I know of no experiment, by which the viſible and permanent effects produced by light, without apparent heat, can be ſo expeditiouſly and ſo diſtinctly exhibited.

AN

INQUIRY

CONCERNING THE

WEIGHT ASCRIBED TO HEAT.

[Read before the Royal Society, May 2, 1799.]

THE various experiments which have hitherto been made with a view to determine the queſtion, ſo long agitated, relative to the weight which has been ſuppoſed to be gained, or to be loſt, by bodies, upon their being heated, are of a nature ſo very delicate, and are liable to ſo many errors, not only on account of the imperfections of the inſtruments made uſe of, but alſo of thoſe, much more difficult to appreciate, ariſing from the vertical currents in the atmoſphere, cauſed by the hot or the cold body which is placed in the balance, that it is not at all ſurpriſing that opinions have been ſo much divided, relative to a fact ſo very difficult to aſcertain.

It is a conſiderable time ſince I firſt began to meditate upon this ſubject, and I have made many experiments with a view to its inveſtigation; and in theſe experiments, I have taken all thoſe precautions to avoid errors, which a knowledge of the various ſources of them, and an earneſt deſire to deter-

determine a fact which I conceived to be of importance to be known, could inspire; but, though all my researches tended to convince me, more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry, that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the Transactions of the Royal Society, Vol. LXXV. made by Dr. FORDYCE, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to *His most Serene Highness the* ELECTOR PALATINE DUKE *of* BAVARIA; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose, —I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related, might be depended on; and, with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner.

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask,—blown as thin as possible,—and of the same shape and dimensions, I chose out from amongst them, two, which, after using every method I could imagine of comparing them together, appeared to be so much alike, as hardly to be distinguished from each other.

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Into one of theſe bottles, which I ſhall call A, I put 4107,86 grains Troy, of pure diſtilled water, which filled it about half full; and into the other, B, I put an equal weight of weak ſpirit of wine; and ſealing both the bottles hermetically, and waſhing them, and wiping them perfectly clean and dry on the outſide, I ſuſpended them to the arms of the balance, and placed the balance in a large room, which for ſome weeks had been regularly heated every day by a German ſtove, and in which the air was kept up to the temperature of 61° of FAHRENHEIT's thermometer, with very little variation. Having ſuffered the bottles, with their contents, to remain in this ſituation till I conceived they muſt have acquired the temperature of the circumambient air, I wiped them afreſh, with a very clean dry cambric handkerchief, and brought them into the moſt exact equilibrium poſſible, by attaching a ſmall piece of very fine ſilver wire to the arm of the balance to which the bottle which was the lighteſt was ſuſpended.

Having ſuffered the apparatus to remain in this ſituation about twelve hours longer, and finding no alteration in the relative weights of the bottles,—they continuing all this time to be in the moſt perfect equillbrium,—I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F; the air without doors being at the ſame time at 27°; and, going out of the room, and locking the door after me, I ſuffered the bottles to remain forty-eight hours, undiſturbed, in this

cold ſituation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room,—uſing the utmoſt caution not to diſturb the balance,—when, to my great ſurpriſe, I found that the bottle A very ſenſibly preponderated.

The water which this bottle contained was completely frozen into one ſolid body of ice; but the ſpirit of wine, in the bottle B, ſhewed no ſigns of freezing.

I now very cautiouſly reſtored the equilibrium, by adding ſmall pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was ſuſpended, when I found that the bottle A had augmented its weight by $\frac{1}{3590 4}$ part of its whole weight at the beginning of the experiment: the weight of the bottle with its contents having been 4811,23 grains Troy, (the bottle weighing 703,37 grains, and the water 4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the oppoſite arm of the balance, to counterbalance it.

Having had occaſion juſt at this time to write to my friend, Sir CHARLES BLAGDEN, upon another ſubject, I added a poſtſcript to my letter, giving him a ſhort account of this experiment, and telling him how "*very contrary to my expectation*" the reſult of it had turned out; but I ſoon after found that I had been too haſty in my communication. Sir CHARLES, in his anſwer to my letter, expreſſed doubts reſpecting the fact; but, before his letter had reached me, I had learned from my own expe-

rience, how very dangerous it is, in philoſophical inveſtigations, to draw concluſions from ſingle experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which ſtill remained at the temperature of 61°), the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the ſurrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen.

This experiment being repeated, gave nearly the ſame reſult, the water appearing, when frozen, to be heavier than in its fluid ſtate; but ſome irregularity in the manner in which the water loſt the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as alſo a ſenſible difference in the quantities of weight apparently acquired in the different experiments, led me to ſuſpect, that the experiment could not be depended on for deciding the fact in queſtion; I therefore ſet about to repeat it, with ſome variations and improvements; but before I give an account of my further inveſtigations relative to this ſubject, it may not be amiſs to mention the method I purſued for diſcovering whether the appearances mentioned in the foregoing experiments might not ariſe from the imperfections of my balance; and it may likewiſe be proper to give an account, in this place, of an intermediate experiment which I made, with a view to

diſcover, by a ſhorter route, and in a manner leſs exceptionable than that above mentioned, whether bodies actually loſe, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My ſuſpicions reſpecting the accuracy of the balance aroſe from a knowledge,—which I acquired from the maker of it,—of the manner in which it was conſtructed.

The three principal points of the balance having been determined, as nearly as poſſible, by meaſurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finiſhed, and its two arms brought to be in equilibrio, the balance was proved, by ſuſpending weights, which before were known to be exactly equal, to the ends of its arms.

If with theſe weights the balance remained in equilibrio, it was conſidered as a proof that the beam was juſt; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, ſuſpended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with a file, the thickneſs of the arm which had been lengthened.

Though, in this method of conſtructing balances, the moſt perfect equality in the lengths of the arms may be obtained, and conſequently the greateſt poſſible accuracy, when uſed at a time when the temperature of the air is the ſame as when the

balance was made, yet, as it may happen, that in order to bring the arms of the balance to be of the ſame length, one of them may be much more hammered than the other, I ſuſpected it might be poſſible that the texture of the metal forming the two arms might be rendered ſo far different, by this operation, as to occaſion a difference in their expanſions with heat; and that this difference might occaſion a ſenſible error in the balance, when, being charged with a great weight, it ſhould be expoſed to a conſiderable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, aroſe in any degree from this cauſe, I had only to repeat the experiment, cauſing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a ſenſible difference in the reſults of different repetitions of the ſame experiment, made as nearly as poſſible under the ſame circumſtances, and as it was above all things of importance to aſcertain the accuracy of my balance, I preferred making a particular experiment for that purpoſe.

My firſt idea was, to ſuſpend to the arms of the balance, by very fine wires, two equal globes of glaſs, filled with mercury, and, ſuffering them to remain in my room till they ſhould have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have ſeen if they ſtill remained in equilibrio, under ſuch difference of temperature; but, conſidering the obſtinacy with which moiſture adheres to the ſurface

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of glaſs, and being afraid that, ſomehow or other, notwithſtanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increaſed; and having found by a former experiment, of which an account is given in one of the preceding papers, (that on the moiſture abſorbed from the atmoſphere by various ſubſtances,) that the gilt ſurfaces of metals, do not attract moiſture (ſee page 264;) inſtead of the glaſs globes filled with mercury, I made uſe of two equal ſolid globes of braſs, well gilt and burniſhed, which I ſuſpended to the arms of the balance, by fine gold wires.

Theſe globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61°) of my room, in which they were expoſed more than twenty-four hours, were brought into the moſt ſcrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The reſult of this trial furniſhed the moſt ſatisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts reſpecting the accuracy of my balance, I now reſumed my inveſtigations relative to the augmentation of weight which fluids have been ſaid to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as poſſible againſt every ſource of error and deception. The bottles being of the ſame ſize, neither any occaſional alteration in the preſſure of the atmoſphere during the experiment, nor the neceſſary and unavoidable difference in the denſities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their ſhapes and their quantities of ſurface being the ſame, and as they remained for ſuch a conſiderable length of time in the heat and cold to which they were expoſed, I flattered myſelf that the quantities of moiſture remaining attached to their ſurfaces, could not be ſo different as ſenſibly to effect the reſults of the experiments.—But, in regard to this laſt circumſtance, I afterwards found reaſon to conclude that my opinion was erroneous.

Admitting the fact ſtated by Dr. FORDYCE,—(and which my experiments had hitherto rather tended to corroborate than to contradict,)—I could not conceive any other cauſe for the augmentation of the apparent weight of water, upon its being frozen, than the loſs of ſo great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loſs of latent head added to the weight of one body, it muſt of neceſſity produce the ſame effect on another, and conſequently, that the augmentation of the quantity of latent heat muſt,—in all bodies,—and in all caſes, —diminiſh their apparent weights.

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To determine whether this is actually the caſe or not, I made the following experiment:

Having provided two bottles, as nearly alike as poſſible, and in all reſpects ſimilar to thoſe made uſe of in the experiments above mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and, ſealing them hermetically, and ſuſpending them to the arms of the balance, I ſuffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained twenty-four hours.—But there was not the leaſt appearance of either of them acquiring, or loſing, any weight.

Here it is very certain, that the quantity of heat loſt by the water, muſt have been very conſiderably greater than that loſt by the mercury; the ſpecific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat loſt, produced no ſenſible difference on the weights of the fluids in queſtion.

Had any difference of weight really exiſted, had it been no more than *one millionth* part of the weight of either of the fluids, I ſhould certainly have diſcovered it;—and, had it amounted to ſo much as $\frac{1}{1000000}$ part of that weight, I ſhould have been able to have meaſured it; ſo ſenſible, and ſo very accurate, is the balance which I uſed in theſe experiments.

I was now much confirmed in my ſuſpicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, aroſe from ſome accidental cauſe; but I was not able to conceive what that cauſe could poſſibly be,—unleſs it were, either a greater quantity of moiſture attached to the external ſurface of the bottle which contained the water, than to the ſurface of that containing the ſpirits of wine,—or ſome vertical current or currents of air, cauſed by the bottles, or one of them not being exactly of the temperature of the ſurrounding atmoſphere.

Though I had foreſeen, and, as I thought, guarded ſufficiently againſt, theſe accidents,—by making uſe of bottles of the ſame ſize and form,—and which were blown of the ſame kind of glaſs,—and at the ſame time,—and by ſuffering the bottles, in the experiments, to remain for ſo conſiderable a length of time expoſed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of ſpirit of wine with reſpect to heat; or, in other words,—the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are expoſed;—or the time taken up in that operation; and, conſequently, was not *abſolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with ſuch variations as ſhould put the matter in queſtion out of all doubt.

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I was the more anxious to aſſure myſelf of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by cauſing unequal currents in the air, but alſo, by cauſing, at the ſame time, a greater or leſs quantity of moiſture to remain attached to the glaſs.

To remedy theſe evils, and alſo to render the experiment more ſtriking and ſatisfactory in other reſpects, I proceeded in the following manner:

Having provided three bottles, A, B, and C, as nearly alike as poſſible, and reſembling in all reſpects thoſe already deſcribed; into the firſt, A, I put 4214,28 grains of water, and a ſmall thermometer, made on purpoſe for the experiment, and ſuſpended in the bottle in ſuch a manner that its bulb remained in the middle of the maſs of water; into the ſecond bottle, B, I put a like weight of ſpirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

Theſe bottles, being all hermetically ſealed, were placed in a large room,—in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being ſuffered to remain in this ſituation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as poſſible, and the contents of the bottles A and B appearing, by their incloſed thermometers, to be exactly at the ſame temperature, the bottles were all wiped with a very clean dry cambric handkerchief; and, being afterwards ſuffered to remain expoſed to the free air of

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the room a couple of hours longer, in order that any inequalities in the quantities of heat,—or of the moiſture attached to their ſurfaces,—which might have been occaſioned by the wiping, might be corrected by the operation of the atmoſphere by which they were ſurrounded, they were all weighed, and were brought into the moſt exact equilibrium with each other, by means of ſmall pieces of very fine ſilver wire, attached to the necks of thoſe of the bottles which were the lighteſt.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were ſuffered to remain, perfectly at reſt and undiſturbed, forty-eight hours; the bottles A and B being ſuſpended to the arms of the balance, and the bottle C ſuſpended, at an equal height, to the arm of a ſtand conſtructed for that purpoſe, and placed as near the balance as poſſible, and a very ſenſible thermometer ſuſpended by the ſide of it.

At the end of forty-eight hours,—during which time the apparatus was left in this ſituation,—I entered the room, opening the door very gently, for fear of diſturbing the balance; when I had the pleaſure to find the three thermometers,—*viz.* that in the bottle A, (which was now incloſed in a ſolid cake of ice,)—that in the bottle B,—and that ſuſpended in the open air of the room, all ſtanding at the ſame point, 29° F, and the bottles A and B *remaining in the moſt perfect equilibrium.*

To aſſure myſelf that the play of the balance was free, I now approached it very gently, and cauſed it to vibrate; and I had the ſatisfaction to find,—

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not only that it moved with the utmoſt freedom,—but alſo,—when its vibration ceaſed,—that it reſted preciſely at the point from which it had ſet out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewiſe remained of the ſame apparent weight as at the beginning of the experiment, being in the ſame perfect equilibrium with the bottle A as at firſt.

I afterwards removed the whole apparatus into a warm room, and cauſing the ice in the bottle A to thaw, and ſuffering the three bottles to remain till they and their contents had acquired the exact temperature of the ſurrounding air, I wiped them very clean, and comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated ſeveral times, and always with preciſely the ſame reſult; the water, *in no inſtance*, appearing to gain, or to loſe, the leaſt weight, upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the leaſt changed, by the various degrees of heat, and of cold, to which they were expoſed.

If the bottles were weighed at a time when their contents were not *preciſely of the ſame temperature*, they would frequently appear to have gained, or to have loſt, ſomething of their weights;—but this doubtleſs aroſe from the vertical currents which they cauſed in the atmoſphere, upon being heated or cooled in it; or to unequal quantities of moiſture attached

attached to the ſurfaces of the bottles;—or to both theſe cauſes operating together.

As I knew that the conducting power of mercury, with reſpect to heat, was conſiderably greater than either that of water, or that of ſpirit of wine, while its capacity for receiving heat is much leſs than that of either of them, I did not think it neceſſary to incloſe a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the other two bottles ſhould appear, by their thermometers, to have arrived at the temperature of the medium in which they were expoſed, the contents of the bottle C could not fail to have acquired it alſo, and even to have arrived at it before them; for the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat, *directly*, and as its conducting power, *inverſely*.

The bottles were ſuſpended to the balance by ſilver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glaſs. I likewiſe avoided, upon all occaſions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or loſe any weight, upon being changed from a ſtate of *fluidity* to that of *ice*, and *vice verſâ*, I ſhall now take my final leave of a ſubject which has long

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occupied me, and which has coſt me much pains and trouble; being fully convinced, (from the reſults of the above-mentioned experiments,) that if heat be in fact a *ſubſtance*, or matter,—a fluid *ſui generis*, as has been ſuppoſed,—which, paſſing from one body to another, and being accumulated, is the immediate cauſe of the phænomena we obſerve in heated bodies, (of which, however, I cannot help entertaining doubts,) it muſt be ſomething ſo infinitely rare, even in its moſt condenſed ſtate, as to baffle all our attempts to diſcover its gravity. And, if the opinion which has been adopted by many of our ableſt philoſophers, that heat is nothing more than an inteſtine vibratory motion of the conſtituent parts of heated bodies, ſhould be well founded, it is clear that the weights of bodies can in no wiſe be affected by ſuch motion.

It is, no doubt, upon the ſuppoſition that heat is a ſubſtance diſtinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken, with a view to determine the weight which bodies have been ſuppoſed to gain, or to loſe, upon being heated or cooled, have been made; and, upon this ſuppoſition (but without, however, adopting it entirely, as I do not conceive it to be ſufficiently proved,) all my reſearches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable;—in which no foreign cauſe whatever could affect the reſults of them;—and the quantity of heat which water is known to part with,

upon

upon being frozen, is ſo conſiderable, that if this loſs has no effect upon its apparent weight, it may be preſumed that we ſhall never be able to contrive an experiment by which we can render the weight of heat ſenſible.

Water, upon being frozen, has been found to loſe a quantity of heat amounting to 140 degrees of FAHRENHEIT's thermometer; or,—which is the ſame thing,—the heat which a given quantity of water, previouſly cooled to the temperature of freezing, actually loſes, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raiſe it to the temperature of (32° + 140) 162° of FAHRENHEIT's thermometer, which is only 60° ſhort of that of boiling water; conſequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loſes almoſt as much heat as, added to it, would be ſufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid ſtate, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was very nearly equal to that, between water in a ſtate of boiling, and the ſame at the temperature of freezing.

But this quantity of heat will appear much more conſiderable, when we conſider the great capacity of water to contain heat, and the great apparent effect which the heat that water loſes upon being frozen

frozen would produce, were it to be imbibed by, or communicated to, any body whoſe power of receiving and retaining heat is much leſs.

The capacity of water to receive and retain heat, —or what has been called its ſpecific quantity of latent heat,—has been found to be to that of gold as 1000 to 50,—or as 20 to 1; conſequently, the heat which any given quantity of water loſes upon being frozen,—were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, inſtead of being heated 162 degrees, would be heated $140 \times 20 = 2800$ degrees, or, would be raiſed to a *bright red heat.*

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about $9\frac{3}{4}$ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no ſenſible effect upon a balance capable of indicating ſo ſmall a variation of weight as that of $\frac{1}{1000000}$ part of the body in queſtion; and, if the weight of gold is neither augmented nor leſſened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very ſafely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES, WILL BE FRUITLESS.

SUPPLEMENT.

THE foregoing paper having been originally drawn up for the purpoſe of being laid before the Royal Society, my reſpect for that learned body induced me to confine my obſervations to ſuch points as I conceived to be new; and I took no notice whatever of a conſiderable number of experiments which I had made in the courſe of my inveſtigations, becauſe they were very ſimilar to experiments that had before been made by other perſons; and becauſe their reſults did not appear to me to afford ſufficient grounds to form any deciſive opinion reſpecting the matter in queſtion. There were, however, among my experiments, two or three, of which I ſhall now give an account, which will probably be thought ſufficiently intereſting to deſerve being mentioned.

Moſt of the experiments, from the reſults of which philoſophers had been induced to form their opinions, reſpecting the *ponderability of heat*, had been made by weighing the ſame given body at different temperatures. Thus, ſolid globes of metal, cannon balls for inſtance, had frequently been weighed when cold, and then, being heated red-hot, had been again weighed at that high temperature, and, from the apparent difference of the weight of the ball when cold, and when red-hot, concluſions had been formed reſpecting the weight, or levity of heat.—But had the numerous cauſes of

error in theſe moſt difficult experiments been leſs evident than they are, yet the reſults of the experiments of this kind which have hitherto been made, by different perſons, have been ſo various and contradictory, that no reliance whatever can be placed on them.

When a hot body is ſuſpended in the air to the arm of a balance, in order to its being weighed, as it continually gives off heat to the fluid in contact with it, this communication of heat occaſions a ſtrong aſcending current of air to be formed over and by the ſides of the hot body, which current cannot fail to affect the reſult of the experiment, and render the concluſions drawn from it fallacious. To prevent, if poſſible, theſe cauſes of error, the following experiments were contrived.

The hot body to be weighed, which was a ſmall metallic ball, heated red-hot, was placed in the ſcale of the balance in a ſmall hemiſpherical porcelain cup, which had a ſlender foot, or ſtand, about one inch high; and this cup, with the hot ball in it, was covered over by a porcelain coffee-cup, turned upſide down, which, without touching the hot ball, confined the heated air which ſurrounded it. This coffee-cup, and the porcelain ſtand, were very exactly balanced, by weights in the oppoſite ſide, before the ball was introduced.

The following experiment was made at Munich on the 20th of April, 1785: The weather being cloudy, with intervals of ſun-ſhine, the thermometer in my room ſtood at 52° F. and the barometer 26 inches 4 lines, French meaſure.

At 30 *minutes after noon*, a ſmall bullet, or grape-ſhot, of caſt iron,—very well formed,—and apparently ſolid, having been well waſhed and cleaned by ſcowering with ſand, and thoroughly dried, was expoſed in a clean veſſel of porcelain in the midſt of a mixture of pounded ice and ſea ſalt, till it had acquired the temperature of 25° F. (7 degrees below the point of freezing water,) when it was carefully weighed, and found to weigh very exactly $773\frac{15}{64}$ grains troy.

At 1*h*. 30*m*. *P*. *M*. the ſame bullet having been expoſed 30 minutes in a clean dry veſſel of porcelain, placed in a ſand heat, at the temperature of 212° F. or that of boiling water, was again weighed, while yet hot, and found to weigh no more than $773\frac{5}{64}$ grains.

At 2*h*. 0*m*. *P*. *M*. the bullet having now been expoſed 15 minutes, in a clean new Heſſian crucible, well covered, to the heat of a ſtrong charcoal fire, and being thoroughly red-hot, was found to weigh $773\frac{31}{64}$ grains.

The bullet being yet red-hot, was put again into the crucible, and being once more expoſed to the fire, which now burned very bright, *at* 2*h*. 20*m*. it had acquired a white heat, and began to ſhew ſigns of melting, ſome ſmall bubbles appearing upon its ſurface. In this ſtate it was taken from the fire, and very carefully weighed 16 times, ſucceſſively, at different intervals, when it was found to weigh as follows.

Time

Time when weighed.				Was found to weigh.
At 2h.	20m.	-	-	$773\frac{35}{64}$ Grains.
2	21	-	-	$773\frac{23}{64}$
2	23	-	-	$773\frac{13}{64}$
2	26	-	-	$772\frac{61}{64}$
2	29	-	-	$773\frac{3}{64}$
2	32	-	-	$773\frac{15}{64}$
2	43	-	-	$773\frac{63}{64}$
2	46	-	-	$774\frac{7}{64}$
2	49	-	-	$774\frac{11}{64}$
2	52	-	-	$774\frac{13}{64}$
2	56	-	-	$774\frac{19}{64}$
2	58	-	-	$774\frac{23}{64}$
3	1	-	-	$774\frac{25}{64}$
3	18	-	-	$774\frac{39}{64}$
* 3	25	-	-	$774\frac{34}{64}$
6	15	-	-	$774\frac{69}{64}$

Immediately after this laſt-mentioned weighing of the bullet, the whole of the apparatus appearing to have acquired the temperature of the air in the room, 60° F. the bullet was taken away, and the porcelain ſtand and cup were again balanced in the ſcale, when it appeared that they had loſt $\frac{1}{4}$ of a grain in weight during the preceding experiment.

* Juſt before this weighing, the coffee-cup which covered the bullet in the ſcale had been removed for a moment, to look at the bullet, and then immediately replaced. What it was that eſcaped on this occaſion, I will not undertake to ſay, but certain it is, that its weight amounted to $\frac{5}{64}$ of a grain at the leaſt.

This apparent loſs of weight I could aſcribe to nothing but to the thorough drying of the cups in the experiment with the red-hot bullet, and to the drying of the ſilk cords by which the ſcale containing the cup and ſtand was ſuſpended to the arm of the balance.

This weight $= \frac{1}{4}$ of a grain, which was required to balance the ſcales at the end of the experiment, being added to the apparent weight of the bullet at 6h. 15m. $= 774\frac{61}{64}$, its true weight at that time appears to have been $775\frac{13}{64}$.

The weight of the bullet at 1h. 30m. having been no more than $773\frac{5}{64}$ grains, and at 6h. 15m. it being found to weigh $775\frac{13}{64}$ grains, it appears that it had gained in weight by being heated red-hot $775\frac{13}{64} - 773\frac{5}{64} = 2\frac{1}{8}$ grains.

This augmentation of weight doubtleſs aroſe from the partial oxidation of the iron. It certainly did not ariſe *from the heat*, for it remained after the bullet had become cold.

An Account of an Experiment made with a Bullet of fine Gold.

Munich, 23*d April* 1785.—Weather cloudy,—with intervals of ſun-ſhine;—thermometer in my room at 65° F.—barometer at 26 inches 4 lines.

A ſmall bullet of fine gold, equal in value to 10 German ducats, which I procured from the maſter of the mint, being weighed in the open air of my room, was found to weigh $477\frac{1}{2}\frac{57}{6}$ grains.

The

The ſmall open china cup, in which the bullet was weighed, was exactly counterbalanced by a weight = $440\frac{2}{2}\frac{4}{5}\frac{8}{6}$ grains.

At 10h. 5m. A. M. the bullet, heated to a clear red heat, approaching to whiteneſs, and weighed in the cup, open to the air, the bullet and the cup together were found to have loſt of their weight $\frac{1}{2}\frac{2}{5}\frac{3}{6}$ of a grain.

Removing the bullet immediately, I found that the cup, or rather the cup and the ſcale in which it was placed, together, had loſt in weight $\frac{1}{2}\frac{2}{5}\frac{0}{6}$ parts of a grain.

Conſequently, the bullet muſt have loſt of its weight by being heated red-hot; or it appeared to be lighter when red-hot than when cold by $\frac{3}{2}\frac{}{5}\frac{}{6}$ parts of a grain, or $\frac{1}{4}\frac{}{0}\frac{}{7}\frac{}{4}\frac{}{9}$ part of its whole weight.

Upon repeating the experiment I had nearly the ſame reſult; but upon varying it, by covering the heated bullet in the ſcale, in different ways, I found ſuch variations in the reſults, as convinced me that the apparent diminution of weight above mentioned might eaſily have ariſen from current in the atmoſphere, and conſequently that no dependence can be placed in experiments of that kind, for deciding the fact relative to the weights of heated bodies, or the ponderability of heat.

I afterwards contrived an apparatus for making the experiment in a different and more unexceptionable manner. I provided three hollow globes of braſs, very thin, and one larger than the other, and which being made to open in the middle, like a

tobacco-

tobacco-box, could be placed one within another. In the centre globe I intended to place a ſolid bullet of pure gold, red-hot.—Between the centre globe and that next it, I propoſed to leave a ſpace equal to the diameter of the heated bullet, filled with air; and the ſpace between the ſecond globe and the third, I meant to have filled with pounded ice; and I propoſed to have made the experiment at a time when the heat of the atmoſphere ſhould be juſt equal to that of freezing water; and in this manner I conceived that I ſhould be able to avoid the currents in the air, whoſe effects I had found ſo diſtreſſing in my former experiments. But when I conſidered that the whole of the heat contained by the red-hot bullet would not be ſufficient to thaw one half of the ice which ſurrounded it, and that, when the bullet ſhould be cooled to the temperature of the ice, the whole maſs of metal,—of ice,—and of water, would ſtill remain *at the point of freezing;* and moreover, that, weighing the water produced by the ice would in fact be weighing the heat which before exiſted in the red-hot bullet, it firſt occurred to me that the point in queſtion might much more readily be determined by ſimply weighing a quantity of ice, at the temperature of freezing, and weighing the ſame again when changed into water.

I therefore left my apparatus unfiniſhed, and turned my whole attention to the experiments, of which an account has been given in the former part of this paper.

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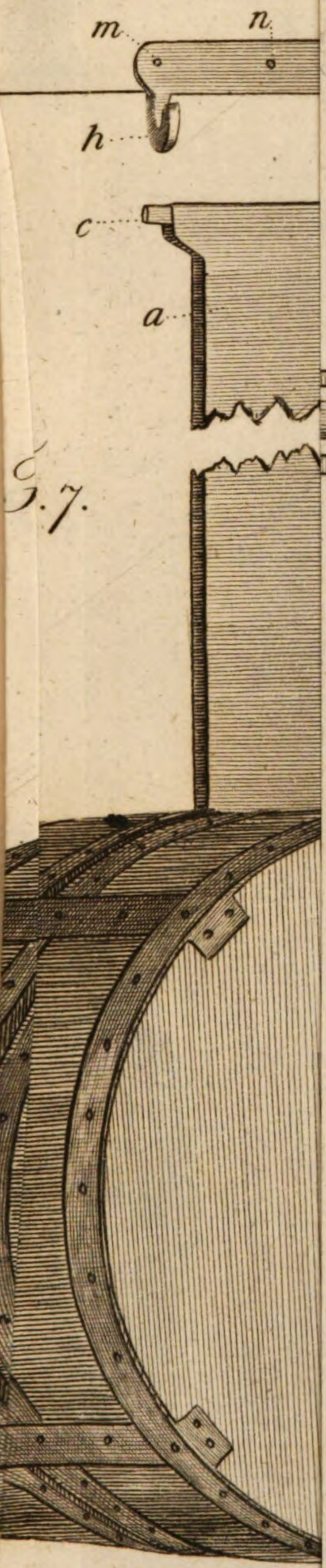

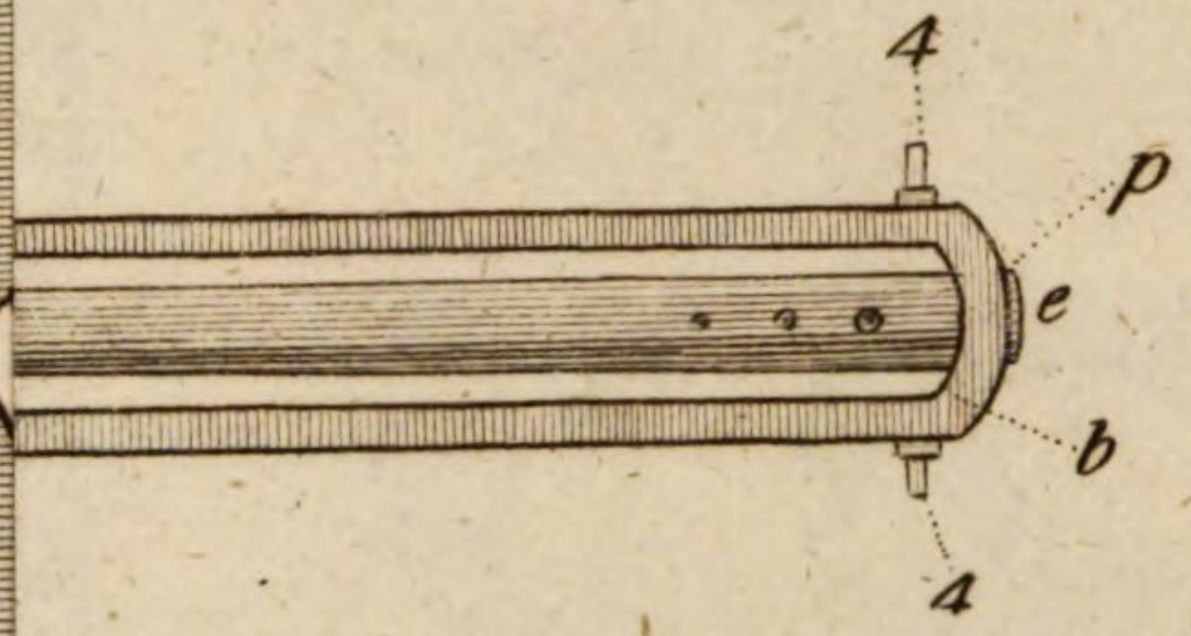

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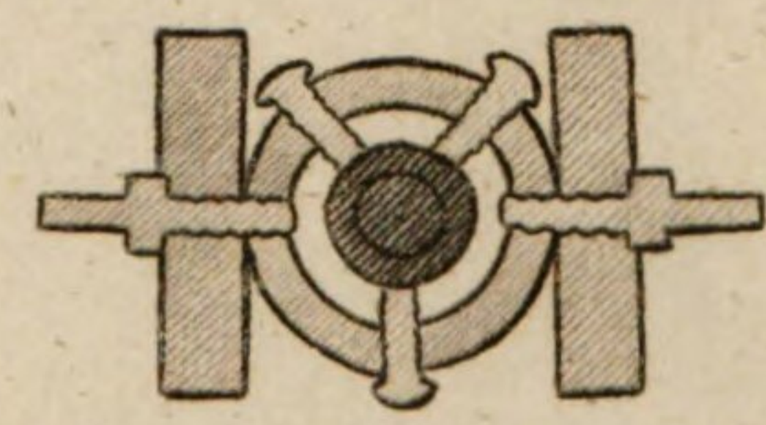

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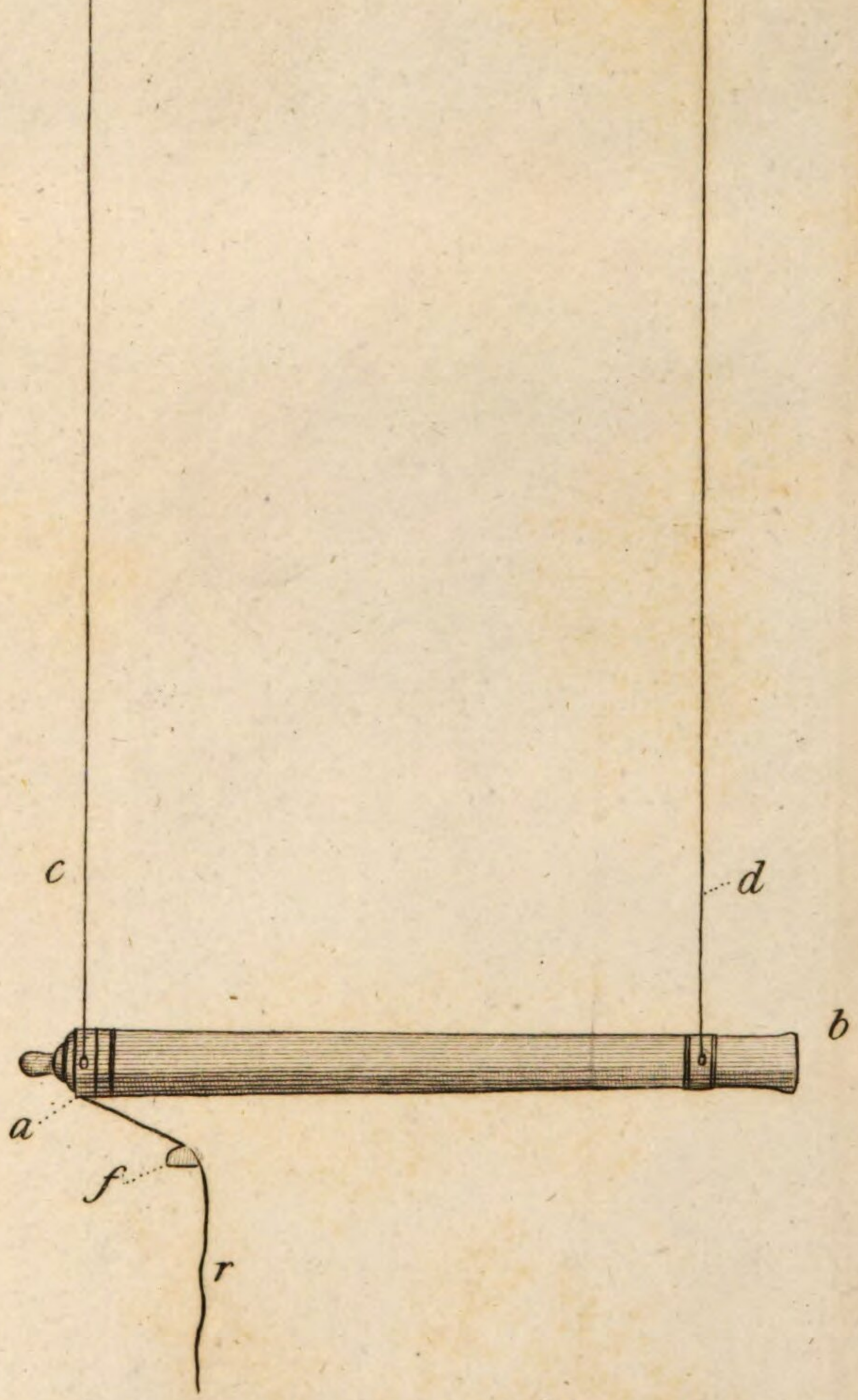

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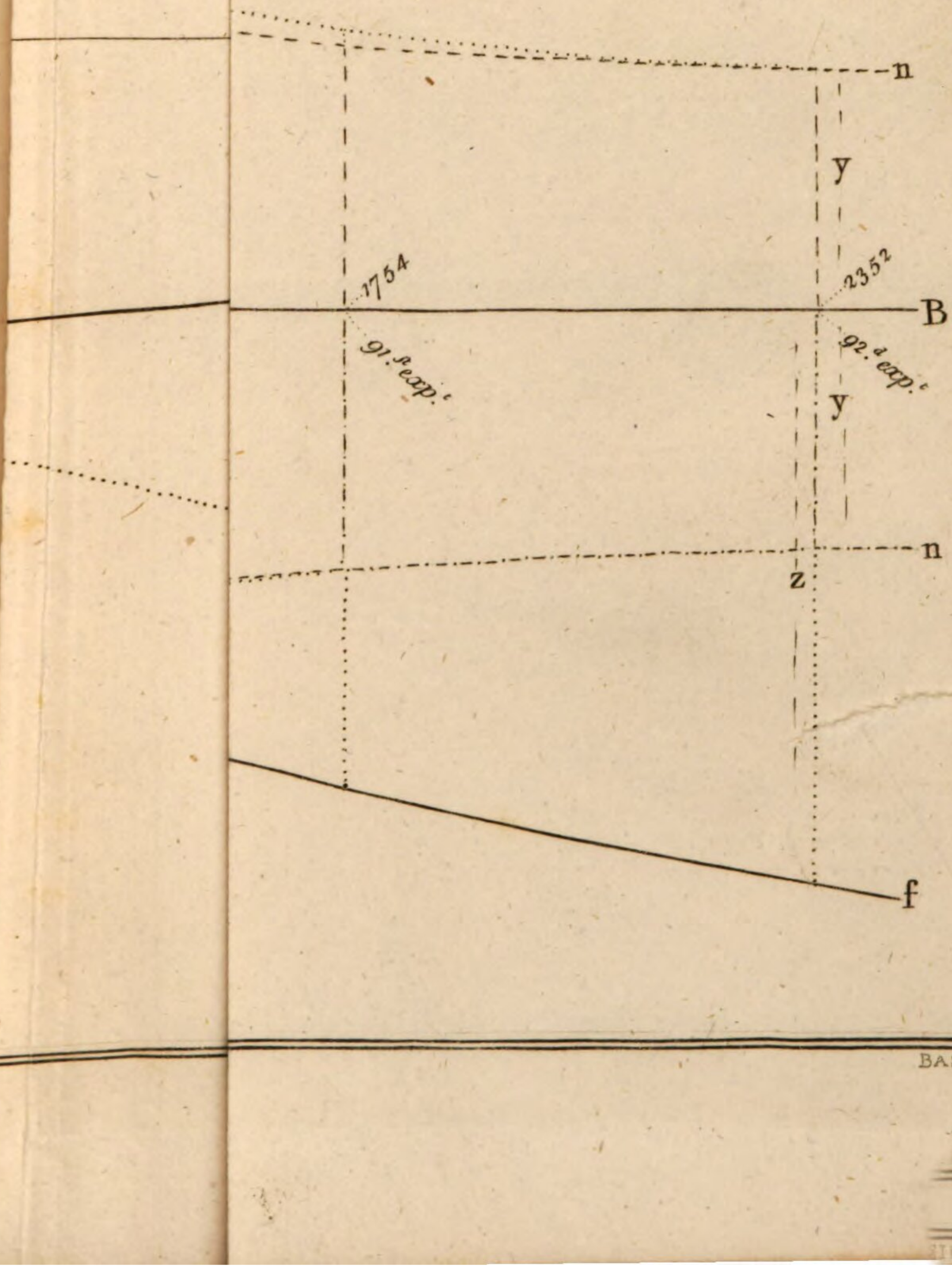

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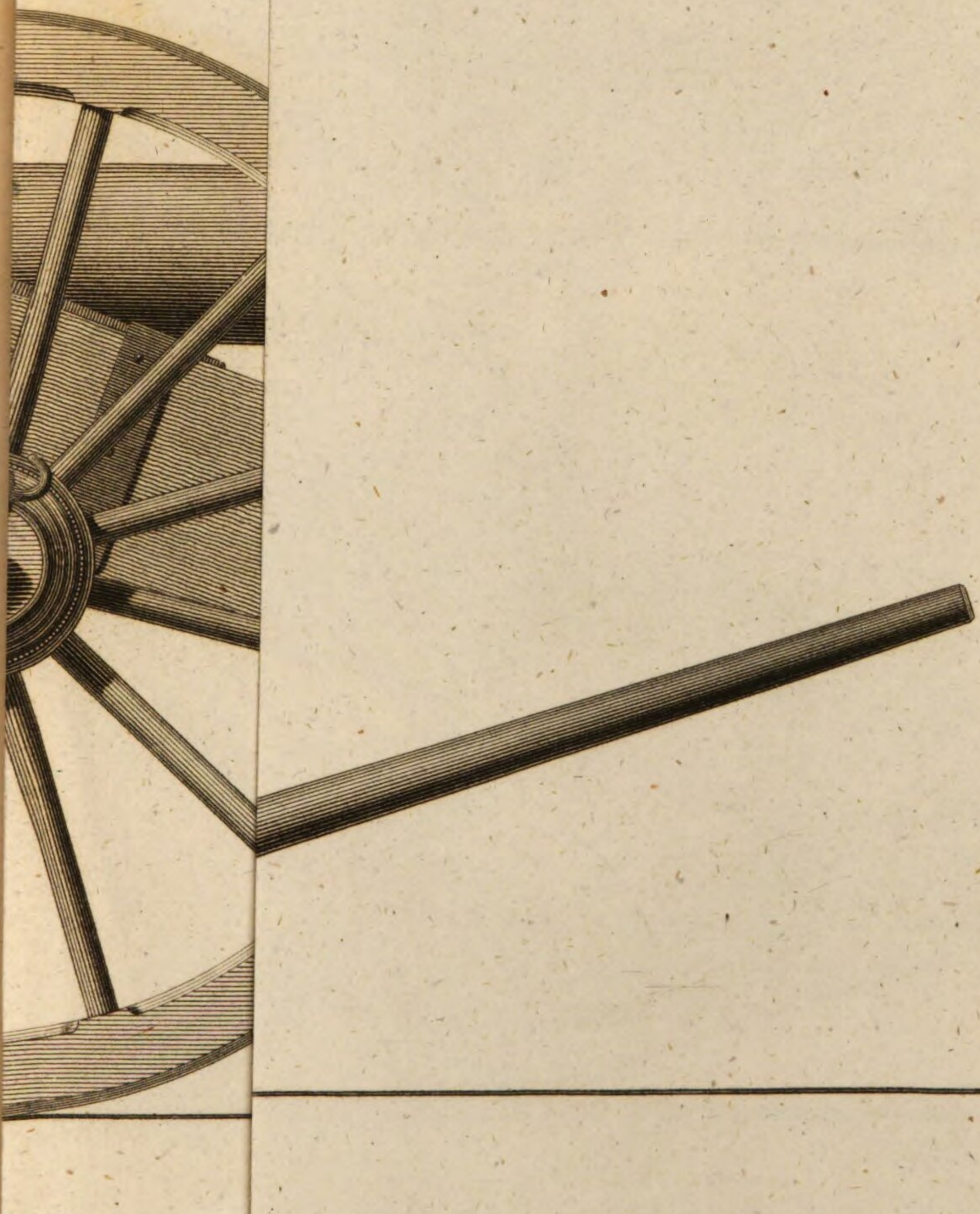

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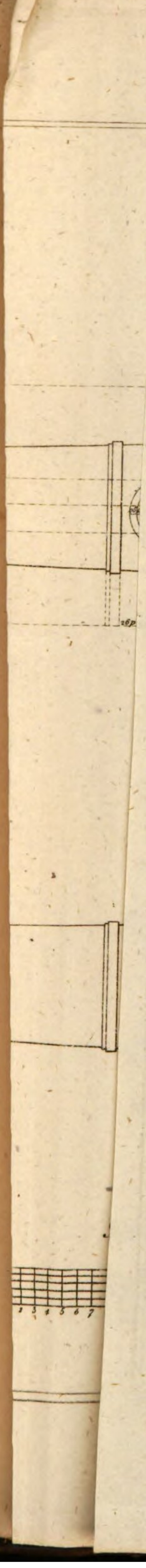

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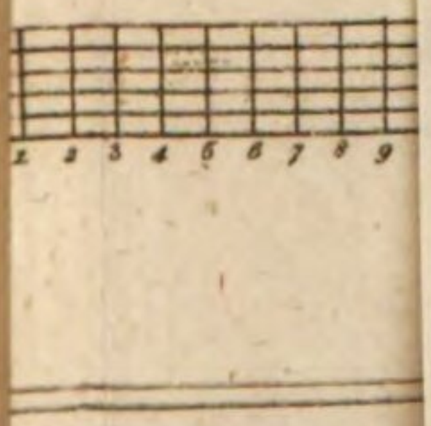

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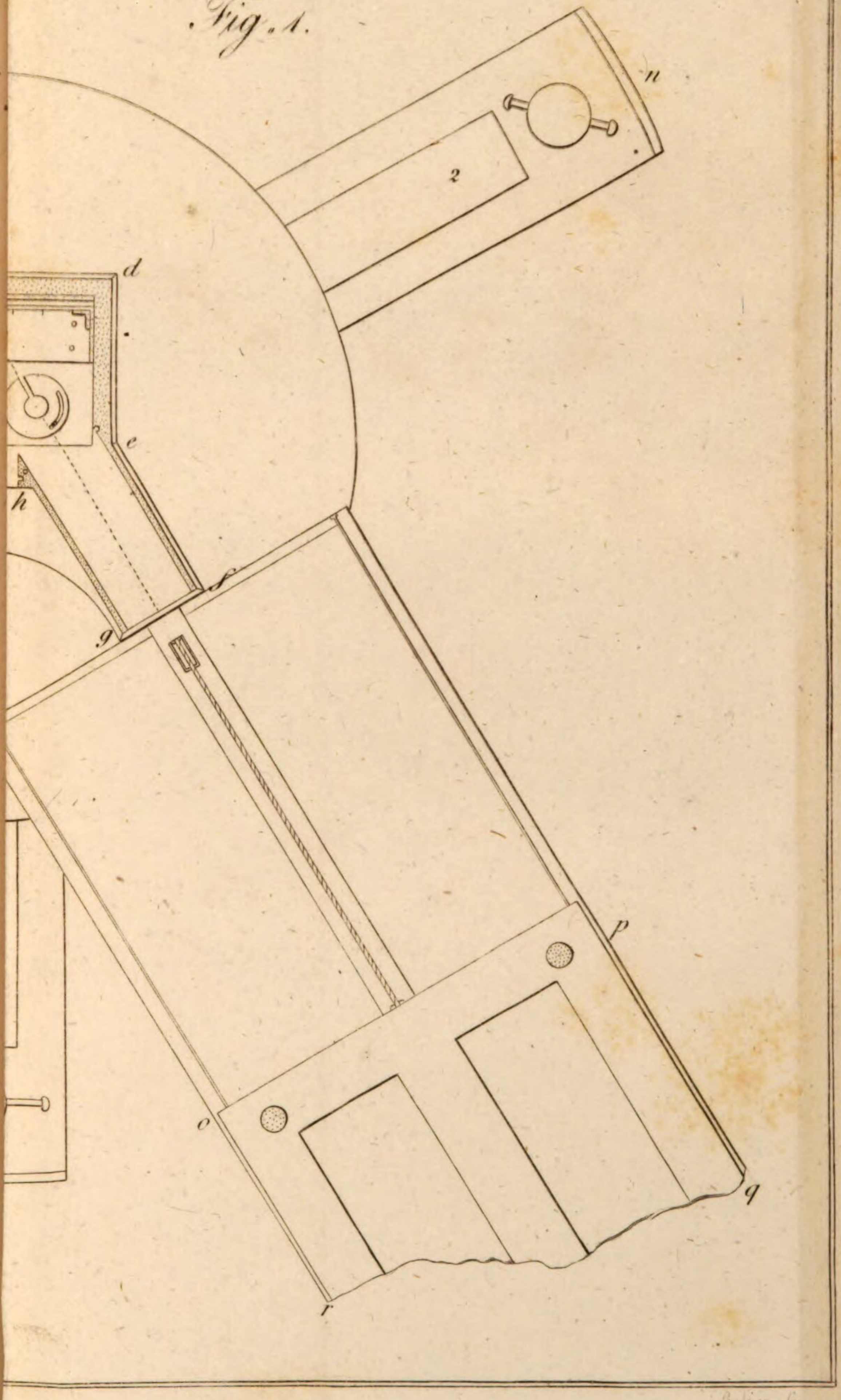
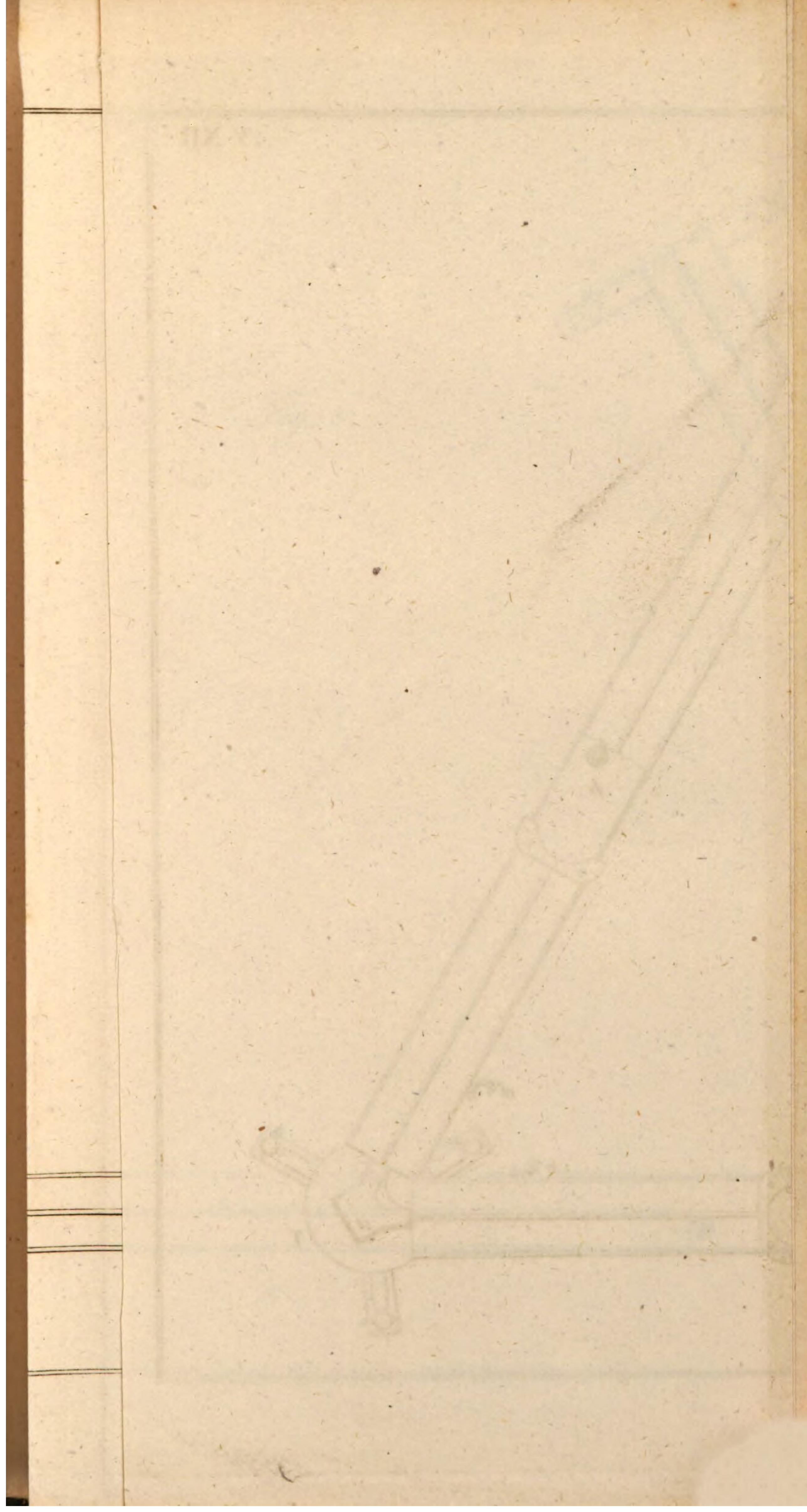
Fig. 1.





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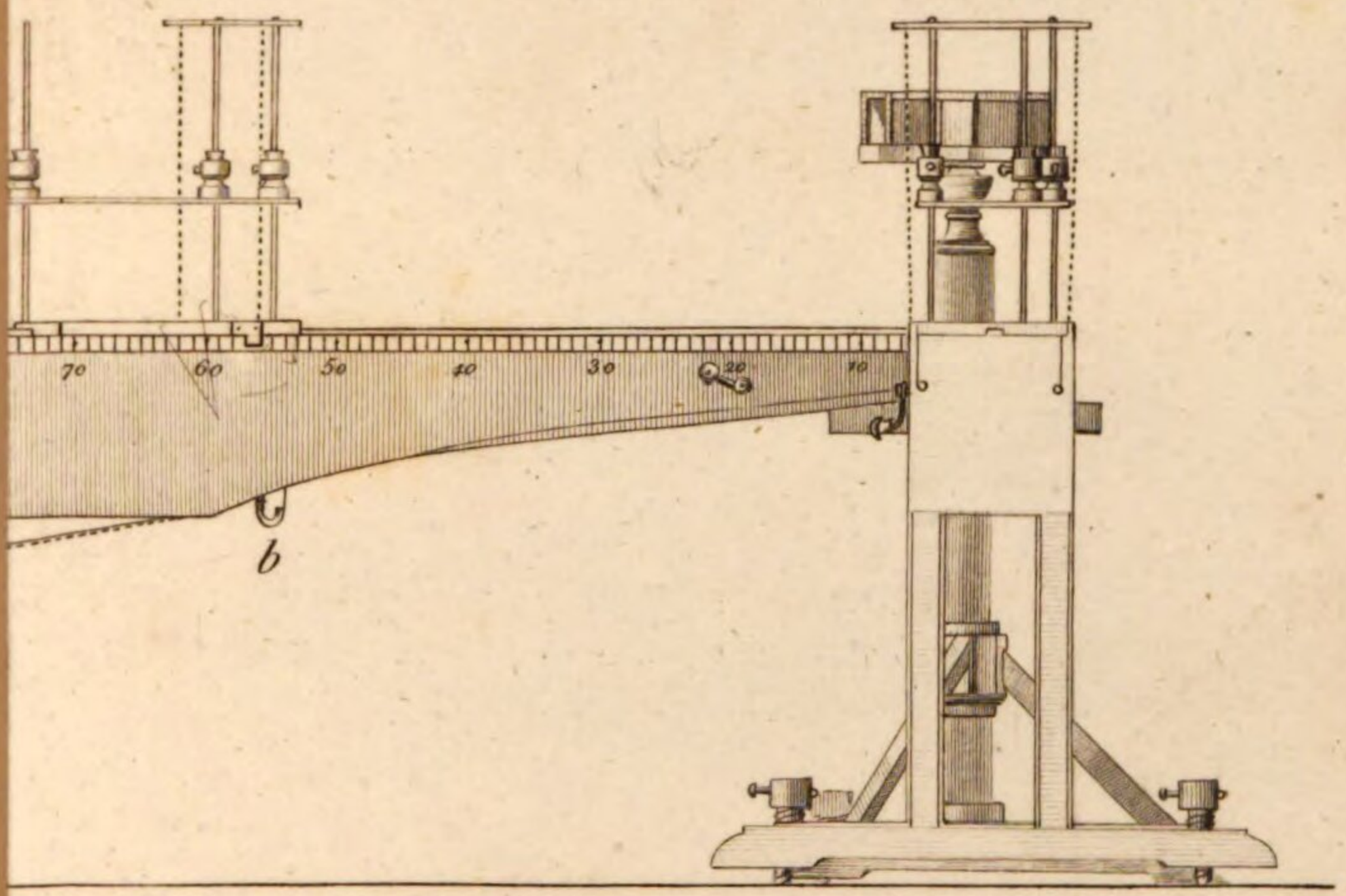

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P3. Qc

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Rumford, Benjamin Thompson <<von>>,

Philosophical Papers: being a collection of memoirs, dissertations, and experimental investigations, relating to various branches of natural philosophy and mechanics
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London 1802

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